

February 1960

AMERICAN SOCIOLOGICAL REVIEW

Sociology and Humane Learning	<i>Robert Bierstedt</i>
Occupational Mobility and Family Cohesion	<i>Eugene Litwak</i>
Illegitimacy in the Caribbean	<i>William J. Goode</i>
Cultural Relativism and Premarital Sex Norms	<i>Harold T. Christensen</i>
Unmarried Fathers and the Mores	<i>Clark E. Vincent</i>
Premarital Characteristics of the Intermarried	<i>Jerold S. Heiss</i>
Recruitment and the Large Law Firm	<i>Erwin O. Smigel</i>
Cultural Occasions and Group Structures	<i>Ralph E. Dakin</i>

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VOLUME 25 NUMBER 1

ARTICLES

Sociology and Humane Learning.....	ROBERT BIERSTEDT	3
Occupational Mobility and Extended Family Cohesion.....	EUGENE LITWAK	9
Illegitimacy in the Caribbean Social Structure.....	WILLIAM J. GOODE	21
Cultural Relativism and Premarital Sex Norms.....	HAROLD T. CHRISTENSEN	31
Unmarried Fathers and the Mores: "Sexual Exploiter" as an Ex Post Facto Label.....	CLARK E. VINCENT	40
Premarital Characteristics of the Religiously Intermarried in an Urban Area....	JEROLD S. HEISS	47
The Impact of Recruitment on the Organization of the Large Law Firm.....	ERWIN O. SMIGEL	56
Cultural Occasions and Group Structures: A Photographic Analysis of American Social Situations.....	RALPH E. DAKIN	66

RESEARCH REPORTS AND NOTES

The Community Social Profile.....	IRWIN T. SANDERS	75
Rural-Urban Fertility Differentials in Mexico.....	WARREN C. ROBINSON AND ELIZABETH H. ROBINSON	77
A Note on Social Mobility, Religiosity, and Students' Attitudes Towards Premarital Sexual Relations.....	FRANK LINDENFELD	81

COMMUNICATIONS

On Values and Value Theory.....	FRANZ ADLER	85
Reply to Adler.....	WILLIAM R. CATTON, JR.	86
On "Democracy and Working-Class Authoritarianism".....	ELKHANAN LAHAV	88
Reply to Lahav.....	SEYMOUR MARTIN LIPSET	89
On Culture and Personality.....	S. KIRSON WEINBERG	91
Reply to Weinberg.....	ROBERT ENDLEMAN	91
On the Modification of Kendall's Coefficient.....	RICHARD H. WILLIS	92
Reply to Willis.....	JAMES A. JONES	92
On Reciprocal Inhibition.....	J. WOLPE	92
Reply to Wolpe.....	THOMAS F. A. PLAUT	93

THE PROFESSION: REPORTS AND OPINION

State Representatives of the Committee on the Profession.....		95
The Sociologist as Medical Educator: A Discussion.....	SAMUEL W. BLOOM, ALBERT F. WESSEN, ROBERT STRAUS, GEORGE G. READER, AND JEROME K. MYERS	95
Courses Preferred for Admission to Graduate Departments of Sociology.....	F. JAMES DAVIS	102
The Contemporary <i>Polish Sociological Review</i>	JIRI KOLAJA	105
On "Textbooks": A Communication.....	LOUIS M. HACKER	107

NEWS AND ANNOUNCEMENTS

Schedule of Annual Meetings of Regional Sociological Societies, 1960.....		108
William Kirk, 1880-1959.....	RAY E. BABER	108
Robert Horniman Dann, 1889-1959.....	HANS H. PLAMBECK	109
Harry Joseph Walker, 1905-1959.....	G. FRANKLIN EDWARDS	109
Sections of the American Sociological Association.....		110

(Continued on following page)

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BOOK REVIEWS

Lerner: <i>The Human Meaning of the Social Sciences</i> . William L. Kolb.....	117
Nelson: <i>Freud and the 20th Century</i> . Ernest van den Haag.....	118
Dowd: <i>Thorstein Veblen</i> . Louis Schneider.....	119
Antoni: <i>From History to Sociology</i> . Werner J. Cahnman.....	120
Demidenko: <i>A Critique of Contemporary Bourgeois Sociology</i> . Konstantin Symmons.....	121
Kahler: <i>The Tower and the Abyss</i> . Robert Bierstedt.....	122
Toynbee: <i>Hellenism</i> . Clyde Kluckhohn.....	122
Rude: <i>The Crowd in the French Revolution</i> . Elinor G. Barber.....	123
Hauser: <i>The Philosophy of Art History</i> . Hugh Dalziel Duncan.....	124
Packard: <i>The Status Seekers</i> . William Petersen.....	124
Whyte: <i>Man and Organization</i> . Robert Dubin.....	126
Dalton: <i>Men Who Manage</i> . Robert Dubin.....	126
Dimock: <i>Administrative Vitality</i> . Robert Dubin.....	126
Abrahamson: <i>A Neighborhood Finds Itself</i> . Nathan Glazer.....	127
Harper and Dunham: <i>Community Organization in Action</i> . Herman D. Stein.....	128
McCord and McCord: <i>Origins of Crime</i> . Lewis Yablonsky.....	128
Albee: <i>Mental Health and Manpower Trends</i> . Howard E. Freeman.....	129
Schachter: <i>The Psychology of Affiliation</i> . John D. Campbell.....	130
Hovland and Janis: <i>Personality and Persuasibility</i> . Donald N. Michael.....	131
Lindzey: <i>Assessment of Human Motives</i> . Morton Deutsch.....	131
Schutt: <i>FIRO</i> . Morton Deutsch.....	131
Karlson: <i>Social Mechanisms</i> . Harold Guetzkow.....	133
Fox: <i>Experiment Perilous</i> . H. Warren Dunham.....	133
Lolli et al.: <i>Alcohol in Italian Culture</i> . Luigi Petruccio.....	134
Bascom and Herskovits: <i>Continuity and Change in African Cultures</i> . Jack Goody.....	135
Udy: <i>Organization of Work</i> . E. R. Leach.....	136
Brameld: <i>The Remaking of a Culture</i> . Robert A. Manners.....	138
Norbeck: <i>Pineapple Town</i> . Clarence E. Glick.....	139
Dore: <i>City Life in Japan</i> . Herbert Passin.....	139
Spiro: <i>Children of the Kibbutz</i> . J. E. Ellemers.....	140
Fichter: <i>Parochial School</i> . Dan C. Lortie.....	141
Giles: <i>The Integrated Classroom</i> . Paul Walter, Jr.....	142
Thompson: <i>Population and Progress in the Far East</i> . George W. Barclay.....	142
Free: <i>Six Allies and a Neutral</i> . Ithie de Sola Pool.....	143
Scott and Withey: <i>The United States and the United Nations</i> . William R. Catton, Jr.....	144
Weiss: <i>Our Public Life</i> . Robert A. Nisbet.....	145
Edel and Edel: <i>Anthropology and Ethics</i> . Richard B. Brandt.....	146
Honigsmann: <i>The World of Man</i> . Andrew P. Vayda.....	146
Jacobson: <i>American Marriage and Divorce</i> . Thomas F. Monahan.....	147
Bierstedt: <i>The Making of Society</i> . Floyd N. House.....	148

BOOK NOTES

Bollens: <i>Special District Governments in the United States</i> . Wendell Bell.....	149
Munch: <i>A Study of Cultural Change</i> . J. A. Barnes.....	149
Crook and Heinstein: <i>The Older Worker in Industry</i> . Stanley H. Udy, Jr.....	149
Centre Interuniversitaire de Sociologie: <i>Etude Analytique de Stratification Sociale</i> . Arnold Levine.....	150
Wach: <i>The Comparative Study of Religions</i> . Norman Birnbaum.....	150
Borgatta and Meyer: <i>Social Control and the Foundations of Sociology</i> . Gisela J. Hinkle.....	150
Habein: <i>Spotlight on the College Student</i> . M. S. O.....	150
Ruml and Morrison: <i>Memo to a College Trustee</i> . M. S. O.....	151
Francis: <i>Wissenschaftliche Grundlagen Soziologischen Denkens</i> . Richard O. Nahrendorf.....	151
Committee on Medical Care Teaching of the Association of Teachers of Preventive Medicine: <i>Readings in Medical Care</i> . M. E. W. G.....	151
Sussman and White: <i>Howe's, Cleveland, Ohio</i> . Christen T. Jonassen.....	151
Coulborn: <i>The Origin of Civilized Societies</i> . Frank H. Hankins.....	152
Dunn: <i>Heredity and Evolution in Human Populations</i> . Frank H. Hankins.....	152
Medeiros: <i>O Processo de Urbanização No Rio Grande do Sul</i> . Janice W. Harris.....	153
Mendras: <i>Les paysans et la modernisation de l'agriculture</i> . M. A. M.....	153
Barbour: <i>A Survey of North West Africa (The Maghrib)</i> . M. A. M.....	153

PUBLICATIONS RECEIVED 154

(Articles in the REVIEW are indexed in the International Index to Periodicals and in the Weekly Bulletin of Public Affairs Information Service.)

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AMERICAN SOCIOLOGICAL REVIEW

February 1960

Volume 25, Number 1

SOCIOLOGY AND HUMANE LEARNING *

ROBERT BIERSTEDT

The City College of New York

The norms governing presidential addresses permit criticism and exhortation—and of this the speaker takes full advantage. Among the criticisms that may be made of contemporary sociology is that it suffers from too narrow a conception of inquiry. The scientific method, however important, does not exhaust the resources of scholarship in sociology. In the present state of our discipline a cogent thesis, informed by a "theoretic bias," is frequently more enlightening than a true hypothesis—even though the latter qualifies as knowledge and the former does not. In any event, sociology owns a proper place not only among the sciences but also among the arts that liberate the human mind.

PRESIDENTIAL addresses, I take it, are ritualistic in character and, whether the association in question be dedicated to the encouragement of barber-shop quartet-singing, the preservation of Manx cats, or the advancement of sociological knowledge, they correspond to other kinds of ritual that may variously appear in a society. Rituals, unlike rites, carry no connotations of secrecy and, like ceremonies in general, they emphasize the special importance of certain events, for example, the annual meeting of the Eastern Sociological Society. The annual meeting itself is a ritual whose primary purpose is to call public attention to the importance of sociology. The meeting has other functions of course: authors seek publishers, publishers seek authors, old members exchange reminiscences in the bar, new members canvass the academic marketplace, and both old and new members occasionally listen to the learned communications of their colleagues.

Now rituals, of course, involve a whole cluster of norms and statuses. The ritual of the presidential address, for example, confers

special privileges and immunities upon the person who occupies the status of speaker and requires special dispensations on the part of those who occupy the status of members of the audience. Among the prerogatives of status is the permission granted to the speaker to indulge in criticism and exhortation to a degree that would, in other circumstances, breach the etiquette of scientific communication. This is a norm to which I intend to conform. The audience in its turn is expected to listen with indulgence and even with patience—provided the address lasts no more than thirty minutes—to almost anything the speaker may want to say. In this respect too—I mean about the thirty minutes—I promise to conform to the norm. The audience, in addition, is not required to agree with anything in the spoken editorial, and certainly not to remember any of it.

But it is time to begin. Let us proceed to the criticisms and exhortations.

We are apt to agree, first of all, that criticism of one's own discipline is a commendable thing, exhibiting as it does a virtuous combination of modesty and sophistication. Sociologists, it has often been observed,

* Presidential address read at the annual meeting of the Eastern Sociological Society, April, 1959.

are humble people, so humble indeed that they are sometimes accused of harboring an inferiority complex. Those of us, doubtless the majority, who express dissatisfaction from time to time about the state of our discipline may be interested to learn that a similar self-questioning can be found elsewhere in the republic of letters. We may even be astonished to read the following in an essay written by Douglas Bush, Professor of English at Harvard: "No one would ever speak of 'the plight of the natural sciences,' or of 'the plight of the social sciences,' but it is always proper to speak of 'the plight of the humanities,' and in the hushed, melancholy tone of one present at a perpetual death bed. For something like twenty-five hundred years the humanities have been in more or less of a plight."¹ In another part of his essay Professor Bush talks, not without irony one may suppose, about the "solid and tangible virtues" of both the natural and the social sciences in contrast to the general lack of esteem accorded the humanities.

Now obviously Professor Bush does not himself believe this about the humanities. Before he finishes his article he restores them to a position of preeminence in the academic hierarchy and dwells at length on the importance of the cultivated mind and of those studies that are "worthy of a free man." I am very glad that he has done this, because I wish to contend in what follows, and contend quite seriously too, that whether or not sociology is or ought to be a science it owns a rightful place in the domain of humane letters and belongs, with literature, history, and philosophy, among the arts that liberate the human mind.

Several years ago, in 1948 to be exact, the late and gentle and much admired Robert Redfield addressed himself to this issue in a lecture entitled "The Art of Social Science," delivered at the University of Chicago.² In this lecture he recognized and paid tribute to those aspects of social science that are scientific in character—in the sense, mainly methodological, that the physical sciences are scientific. But he went on to suggest that

the social sciences ought to be something more and that, indeed, in the highest reaches of their accomplishment they were a great deal more.

Redfield recounts the time when he was a member of a committee of social scientists commissioned first to select outstanding examples of research and then to appraise them from the point of view of the methods employed. The books nominated by the historians and sociologists as of unusual merit were, respectively, *The Great Plains*, by Walter Prescott Webb, and *The Polish Peasant*, by Thomas and Znaniecki. In the process of appraisal "a curious thing happened":

Herbert Blumer, who analyzed *The Polish Peasant* for the committee, came to the conclusion that the method in that book was really unsuccessful because the general propositions set forth in the work could not be established by the particular facts adduced. The committee had to agree. Yet it remained with the impression that this was a very distinguished and important work. Webb's history of cultural transformation in the American West fared no better at the hands of the young historian who analyzed that work. He pointed out many undeniable failures of the author of *The Great Plains* to use and to interpret fully some of the evidence. And yet again a majority of the committee persisted in the impression that Webb's book was truly stimulating, original, and praiseworthy.

Of course, one does not conclude from this experience that the failure of facts to support hypotheses, in whole or in part, is a virtue in social science or is to be recommended. No doubt these books would have been more highly praised had these defects been proved to be absent. But does not the experience suggest that there is something in social science which is good, perhaps essential, apart from success with formal method; that these works have virtues not wholly dependent on the degree of success demonstrated in performing specified and formalized operations on restricted and precisely identified data.³

Redfield goes on to extol the merits of three other treatises in sociology, treatises that must now be regarded as of classical significance in the history of our discipline—Veblen's *The Theory of the Leisure Class*, Sumner's *Folkways*, and Tocqueville's *Democracy in America*. All three of these works are quite innocent of the paraphernalia of formal method and yet all three managed to say something of lasting importance about

¹ Douglas Bush, "The Humanities," *The Key Reporter*, 22 (February, 1955), p. 3.

² *American Journal of Sociology*, 54 (November, 1948), pp. 181-190.

³ *Ibid.*, pp. 181-182.

man in society. Veblen used no questionnaires, Sumner no coefficients of correlation, and Tocqueville was wholly untrained in the modern techniques of field investigation. One does not imply by these examples—and this is necessary to reiterate—that either ignorance or neglect of formal method is a virtue. One does imply that something more than method is required to achieve a genuine superiority. The reason these writers were great sociologists is that they were humanists first, and if they had not been great humanists they could never have become great sociologists.

It has often been said that the social sciences share their subject matter with the humanities and their methods with the sciences. Although this is a suggestive observation, it errs perhaps in sharpening a little too much the distinction between the humanities and the social sciences. For it must be apparent that in the case of the three books mentioned it is difficult to distinguish the sociological from the humane concern. So clear is this to me that I can only wonder at our reluctance in general to acknowledge it. That reluctance does obtain is, I think, beyond dispute. Certainly we do not encourage our students or our younger colleagues to emulate these authors. We usually agree to call such writers sociologists only long after they are dead and would refuse admittance altogether into our guild of those younger men, humanistically inclined, who might want to follow in their footsteps. Imagine what would happen, for example, if one of these three men—Veblen, Sumner, or Tocqueville—were to present himself as a doctoral candidate at any of our leading departments of sociology today with a couple of sample chapters of his *chef-d'oeuvre* under his arm. He would almost certainly be advised to forget the whole thing and to turn instead—if you will forgive the language—to a study of the goal-structures and opportunity-structures of role-oriented actors.⁴

Redfield tells us finally that each of these

three books is an expression of some perception of human nature, that each brings forth significant generalizations, and that each reflects a fresh and independent viewpoint with respect to its subject-matter. These three qualities in turn are functions not of formal method but of the creative imagination of their authors. From these and many more examples of a similar kind we are driven to an unpopular inference. For it seems to be the case that we confront in sociology today the rather odd paradox that the significance of our research varies inversely with the precision of the methods employed. Or, as Redfield put it in another paper on this subject: "The emphasis on formal method sometimes carries the social scientist into exercises in which something not very important is done very well."⁵

Now many of us may accept these consequences with equanimity on the ground that conclusions of modest proportions are the price we pay for precision, and that precision, in turn, is one of the most fundamental requirements of the scientific enterprise. For my own part I have to express what is doubtless a minority view and say that I regard this situation with regret. Is sociology to be a niggling business, doing the easy thing because it is accurate, and avoiding the difficult thing because it is imprecise? We have often been told, and by numerous hostile critics, that sociology is a mean and petty science, pursued by people who take delight in counting the privies in Pittsburgh and discovering, with the most versatile of techniques, that people with high incomes spend more money than people with low incomes. I exaggerate, of course, and it would be invidious in any event to select examples from the literature. But it is distressing to think that sociology can be associated with the solution of problems of a trivial kind and that the more precise our research becomes the more our science resembles the deaf man in Tolstoy, muttering answers to questions that no one has asked him.

As a matter of fact, the versatility and complexity of some of our new techniques may account in part for the public indifference to our discipline. Many of these techniques are so complex, and many of our

⁴ This figure is not original. Arthur M. Schlesinger, Jr., once imagined what would happen if Edward Gibbon were to apply for a grant to a foundation today with the idea of trying to explain the decline and fall of the Roman Empire. He too would be advised either to forget the whole thing or to turn his "project" over to a committee of social anthropologists.

⁵ "Social Science Among the Humanities," *Measurement*, 1 (Winter, 1950), p. 62.

concepts so opaque, that they have interest and meaning only to other sociologists and have no relevance to the society at large. Nor do they produce results that have any claim to a universal attention or a public appreciation. The situation with respect to both methods and concepts sometimes reminds me of the little old lady at the zoo who inquired of the keeper whether the animal in the cage in front of her was a male or a female hippopotamus. "Lady," he replied with dignity, "that is a question that could conceivably be of interest only to another hippopotamus."

An over-emphasis upon facts, of course, can have the same consequences as an over-emphasis upon methods. I have been inclined to wonder on occasion why it is, in contradiction of all of our rules, that those who have been most factual and utilitarian in the history of thought should be so very much less respected in the long run than those who have been theoretical and even speculative in their principal endeavors. Suppose we compare the two French sociologists Frédéric Le Play and Auguste Comte. Le Play was the careful, patient, and diligent investigator, a man who with ingenuity and persistence collected an enormously useful set of facts about the domestic budgets of workers' families in several European countries. Comte on the other hand was a man who collected no facts of any kind, indulged in outrageously speculative dreams about the possibility of a science of society, was guilty of philological bad taste in coining the word "sociology," and invented a ridiculous religion. And yet the faith of a Comte in the possibility of a science of society is more important to us today than the facts of a Le Play and of the two there is little question which has the larger significance in the history of our discipline.

If we cross the English Channel we find a similar situation in the comparison of Charles Booth and Herbert Spencer. Booth, of course, made an exhaustive statistical study of poverty in London and no fewer than seventeen volumes were required to contain his facts—facts of indubitable utility in their time and place. Spencer on the other hand was an egregiously wrong-headed billiard player and philosopher who imposed upon us all an erroneous theory of society

from which it took us several decades to extricate ourselves. Now it is doubtless true, as Crane Brinton once remarked, that no one reads Spencer any more. But no one has even heard of Booth—except, of course, a few antiquarians like ourselves. Once again the philosopher was wrong and the fact-gatherer was right. But what a privilege to be so wrong!

The inference, however unfortunate, is also unmistakable. Books of speculation will be superseded and will remain for us only historical curiosities to be preserved in the museum of the mind. But books of facts succumb even more easily to the lethal challenges of time. Some theories indeed, the "false" as well as the "true," achieve an immortality, whereas the truth that facts possess does not always protect them from an early oblivion.

Our preoccupation with method has still other consequences, not yet mentioned. It frequently dominates our inquiries and determines the kinds of questions we address to society; that is, the method becomes the independent variable, the problem the dependent one. Instead of setting for ourselves tasks of large dimensions and then devising methods appropriate to their solution, we are apt to ask only those questions that are answerable in terms of methods presently available. We have even been invited to forego those larger problems of human society that occupied our ancestors in the history of social thought and to seek instead what T. H. Marshall called, in his inaugural lecture at the University of London, "stepping stones in the middle distance," and other sociologists since, "theories of the middle range." But what an anemic ambition this is! Shall we strive for half a victory? Where are the visions that enticed us into the world of learning in the first place? I had always thought that sociologists too knew how to dream and that they believed with Browning that a man's reach should exceed his grasp.

But enough of criticism. Some of my comparisons, as you no doubt recognize, may be meretricious and all perhaps are exaggerated. They may be meretricious in the sense that it is always easy to compare the brilliant and profound philosopher in sociology with the dull and mediocre statistician, the creative

theorist with the unimaginative researcher. Such comparisons are as illicit as the reverse would be—the ingenious and versatile researcher *versus* the confused or unintelligible theorist. Nor do I have any intention of resurrecting the ancient methodological argument, the one that raged in the pages of our books and journals a couple of decades ago, as to whether or not sociology is or ought to be a science. Let me emphasize for the record my own conviction that sociology ought to be as scientific as it possibly can be, that it ought to conform to all the canons of scientific inquiry, and that conclusions ought to be public and publicly verifiable. When I say that sociology ought to be a science, however, I do not imply that it should be *only* a science. I think we ought to take much more seriously and literally the view that sociology can also serve as a bridge between the sciences and the humanities and that in a very important sense it belongs to the realm of humane letters.

Let me try to speak constructively now and invite your attention to the suggestion that the establishment of true propositions—the scientific task—may not be an altogether satisfactory or even desirable goal for some of our sociological endeavors. Let me illustrate what I mean. In my own opinion one of the greatest pieces of sociological research ever conducted by anyone is Max Weber's *The Protestant Ethic and the Spirit of Capitalism*. And yet none of us, I submit, has any idea whether or not Weber's thesis is "true." It has in fact been criticized, as we all know, by Brentano in Germany, Robertson in Scotland, Tawney in England, Beard in the United States, and Fanfani in Italy. Most of the criticisms are relevant and many are penetrating. We continue, however, in spite of them, to regard Weber's sociology of religion as a distinguished contribution to sociology, one of the most distinguished in the entire literature. Its author's stature as a sociologist is not only not diminished but is positively enhanced by the critical attention his work has received. We admire this work not because it is "true"—indeed its truth escapes all of the ordinary canons of scientific verification—but because of the excellence of its conception, the erudition of its argument, and the general sociological sophistication that informs it. We admire

it, in short, not for its "truth," but rather for its cogency.

Now it may be that we shall have to deny the name of knowledge—the accolade of the label, as it were—to theses of this Weberian kind, and I am perfectly prepared to acquiesce in such a decision. Of course I do not presume to know what knowledge is. My own preference as a solution to this epistemological puzzle is simply to recall the jingle Oxford students used to recite about Benjamin Jowett:

My name is Benjamin Jowett;
I'm the Master of Balliol College
Whate'er can be known, I know it;
And what I don't know is not knowledge.

However difficult an ultimate definition may be, I am sure that all of us would agree that in order to qualify as knowledge a proposition needs to be public in character, the product of shared experience, and verifiable by successive approximations. An item of knowledge then, if not absolutely true, is at least temporarily true in the sense that it has resisted repeated attempts at falsification.

In terms of these criteria, however, it is clear that Weber's observations concerning the relationship between the economic ethic of Protestantism and the development of capitalism do not qualify. The obvious fact, however, that many small researches do qualify for inclusion in the category we call knowledge whereas the massive researches of Max Weber do not must give us pause. In the present condition of our disciplines, and in the foreseeable future, we may be better advised to aim for cogency rather than for truth. It is the cogency of Weber's thesis, and not its truth, that fills us with admiration and that gives it its commanding position in the history of sociological research.

In this connection I should like to introduce another somewhat wayward notion. We have always insisted, with a proper bow to Francis Bacon, upon the elimination of bias in our inquiries and have emphasized the need for as complete an objectivity as it is humanly possible to attain. We have often suggested that in the social sciences, as contrasted with the physical sciences, objectivity is a condition to be achieved and not one initially given in our scientific situation and

that this fact creates greater difficulties for the sociologist, for example, than for his colleagues in physics, chemistry, and biology, and requires perhaps a more alert responsibility. We have realistically recognized, of course, that a complete objectivity, though a methodological *desideratum*, is nevertheless for any individual a psychological impossibility and that what we should hope for is not a total absence of bias but rather an overt awareness of it. Finally, we have admitted in our wiser moments that behind every great sociologist there stands a social philosopher and that not even the scientific sociologist can ultimately escape the ethical and political consequences of his own approach to the problems of society.

All of this is to the good. And yet, I want to suggest the alternative possibility that objectivity may not be as desirable a criterion as it is commonly thought to be. For certain purposes, including the kind of sociological research I have been advocating by implication, it might be preferable to utilize what I shall call "the theoretic bias." The theoretic bias would enable us to push a particular interpretation of social phenomena just as far as it is reasonable to go in our effort to shed illumination upon it.⁶ It would candidly employ exaggeration as an heuristic device. In examining the problems of social change, for example, an objective approach is apt to be a pallid and unsuccessful one. These problems are not in fact amenable to solution with methods currently available. It would seem to be much better, therefore, to take a single factor and to push it to an extreme as a possible mode of interpretation. Thus, Marx used the theoretic bias to support the role of the economic factor, Buckle the geographic factor, Freud the psychological, Weber the ideological, Durkheim the sociological (in a special sense), and so on. Each one of these thinkers was lured into excess by his enthusiasm for his own bias

and each was surely guilty of exaggeration. The greatest thinkers, however, have not been the neutral and objective ones, but those who have turned their biases to good account. And each biased conclusion, of course, is open to refinement, modification, and correction by others of a contrary kind, so that the outcome over the course of time is, if not knowledge in a narrow sense, a much more sophisticated appreciation of the problem than would otherwise be possible. I am inclined to wonder, in short, whether in our assault upon some of the larger problems of sociology biased theses may not serve as better than objective hypotheses.

I hesitate to sound even remotely Hegelian in this connection and hasten to disclaim any metaphysical implications these remarks may seem to contain. All I want to suggest is that successive rebuttal and reaffirmation may be as effective in the treatment of one kind of problem as successive approximation is in another and that in the give and take of argument and counter-argument we have much to gain. In any event, I advocate the theoretic bias on the ground that one of our most imperative needs in contemporary sociology is not more theory, in the sense in which our theory has recently developed, but more theses—that is, positions advanced, taken, defended, lost, and won again in the eternal dialectic that is the life of the mind. The result may not be knowledge, but the reward can nevertheless be great if it helps us to construct a sociology that is responsive to the intellectual challenges of our time.

I have been constrained in these sentences to emphasize that scientific method, as important and indeed as necessary as it is, does not exhaust the resources of scholarship in sociology and that, as we aspire for significance, objectivity and the pursuit of truth may have less to offer us than the theoretic bias and the search for cogency. You may of course reject the criticisms that led to this conclusion and ignore the exhortations. But I should still maintain, in brief conclusion, that sociology has an honorable place in the realm of humane letters and that it belongs with the liberal arts as well as with the sciences. We have seldom been able to escape the public belief that it is the principal business of sociology to solve social problems; and the identification of our discipline with

⁶ The role of reason in sociological research has received insufficient attention in our literature. On this subject see the thoughtful essay by Reinhard Bendix, *Social Science and the Distrust of Reason*, Berkeley and Los Angeles: University of California Press, 1951, esp. pp. 26-42. See also Robert Bierstedt, "A Critique of Empiricism in Sociology," *American Sociological Review*, 14 (October, 1949), pp. 584-592.

such problems is too well known to require comment. That sociology might also have something to do with culture in the narrower and non-sociological sense of intellectual cultivation seems seldom to have occurred to anyone, including sociologists.

I invite your attention, therefore, to the fact that sociology, like the other arts, is one of the ornaments of the human mind, that its literature extending from Plato to our contemporaries is in a great and humane tradition, that sociology—like all of the liberal arts—liberates us from the provincialisms of time and place and circumstance,

that the social order is a study worthy of a free man, and that society itself, like every other thing that has ever agitated the restless and inquisitive mind of man, is a fit and dignified subject of inquiry.

May I say finally that we are easily misled. "It is not the lofty sails but the unseen wind that moves the ship." It is not the methods and the concepts that move our sociology along, but memory and desire—the memory that other men in other times have also asked questions about society and the desire that our answers, in our time, will be better than theirs.

OCCUPATIONAL MOBILITY AND EXTENDED FAMILY COHESION *

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Parsons' hypothesis that extended family relations are antithetical to democratic industrial societies because they are not consonant with occupational mobility is questioned. It is suggested that his hypothesis deals only with the "classical" extended family and industrial societies in their emerging state. It is hypothesized that a "modified" extended family is consonant with occupational mobility in the mature industrial economy. This extended family, by providing aid across class lines, permits the nuclear family to retain its extended family contacts despite differences in class position. At the same time this aid is isolated from the occupational system and does not hinder mobility based on merit. An empirical study of visiting patterns is reported in support of this hypothesis.

IT has been suggested by Talcott Parsons that there is a basic disharmony between modern democratic industrial society and extended family relations. This is in part true, he hypothesizes, because occupational mobility is antithetical to extended family relations.¹ Thus he argues that the isolated nuclear family is especially functional.

The position advanced in this paper is a

modification of Parsons' view. It agrees with him that the "classical"² extended family is antithetical to occupational mobility. However, it is suggested that a modified extended family relation is consonant with occupational mobility and more functional than the isolated nuclear family. Parsons' hypothesis tends to be valid only during periods of emerging industrialization. Because he has so clearly related his hypothesis to a more general theory of social class and organizational efficiency, the modification of his hypothesis has implications for a more general theory of industrial society.³

* This is the first of two companion papers which attempt to document the view that extended family relations remain viable despite mobility—occupational and geographic. The second paper, "Geographical Mobility and Extended Family Cohesion" will be published in a future issue of the *American Sociological Review*. The author wishes to thank Glenn H. Beyer, Director of the Cornell Housing Research Center for permitting use of the data. Paul F. Lazarsfeld and Arthur R. Cohen provided many helpful suggestions.

¹ Talcott Parsons, "Revised Analytical Approach to the Theory of Social Stratification," in R. Bendix and S. M. Lipset, editors, *Class, Status, and Power: A Reader in Social Stratification*, Glencoe: Free Press, 1953, pp. 116 ff.

² The "classical extended" family is here defined in terms of geographical propinquity, occupational dependence and nepotism, a sense that extended family relations are most important, and an hierarchical authority structure based on a semi-biological criterion, i.e., the eldest male.

³ See Parsons, *op. cit.*; and Talcott Parsons, "The Social Structure of the Family," in Ruth N. Anshen, editor, *The Family: Its Function and Destiny*, New York: Harper, 1949.

The paper is divided into two parts. The first attempts to make explicit the assumptions which underlie both the hypothesis suggested here and that made by Parsons. The second presents evidence from an empirical study dealing with the relation between occupational mobility and extended family relations.

ASSUMPTIONS SUPPORTING EXTENDED FAMILY ANOMIE

Behind the hypothesis that extended family relations are antithetical to occupational mobility lie at least two major assumptions: first, the overriding importance of "status by association," and secondly, differential socialization among various occupational strata.

Status achievement, occupational mobility, and extended family dissociation.—It is assumed, first, that status achievement is directly and indirectly related to occupational position. The title of the occupation is itself a status symbol, or the income derived from the occupation permits the purchase of status symbols.⁴ Secondly, it is assumed that status is achieved by associating with those of equal or greater occupational achievement; therefore, the nuclear family which moves up the occupational ladder would lose status if it were to retain close associations with an extended family of lower status (Warner, in his study of "Yankee City," provides excellent illustrations of how this process works.⁵) As a consequence, it is suggested that occupationally mobile individuals are likely to be status oriented and to have little extended family contact or identification.

Differential socialization and extended family dissociation.—Another assumption made by those who posit the contradiction between extended family and occupational mobility is that there are extreme differences in socialization among the various occupational strata.⁶ Under such an assumption, dif-

ferential occupational mobility leads to different socialization experiences among members of extended families. Such differences would be sufficient to disrupt kin relations since family members would have little in common to hold them together.

These well known views are not explored in detail in the following discussion.

ASSUMPTIONS SUPPORTING EXTENDED FAMILY COHESION

The "modified extended" family is the ideal type posited in this paper. This type differs from the "classical extended" family in that it does not demand geographical propinquity, occupational involvement, or nepotism, nor does it have an hierarchical authority structure. On the other hand, it differs from the isolated nuclear family structure in that it does provide significant and continuing aid to the nuclear family. The modified extended family consists of a series of nuclear families bound together on an equalitarian basis, with a strong emphasis on these extended family bonds as an end value. In support of this family type are two assumptions which run counter to those presented above.

Status achievement and extended family cohesion.—The first objection to continued viability of the extended family may be answered by pointing out that people not only achieve status by associating with others of equal or greater social rank but that they also attain it by deference from others.⁷ For instance, a small businessman might achieve status by moving either to an upper-class or to a working-class neighborhood. In the first instance, his sense of status achievement derives from associating with those of equal or higher occupational rank, in the second

⁴ Parsons, "Revised Analytical Approach . . .," *op. cit.*, p. 112.

⁵ W. Lloyd Warner and Paul S. Lunt, *The Social Life of a Modern Community*, New Haven: Yale University Press, 1941, pp. 190-193.

⁶ For one excellent example of a study on class differences, see Herbert Hyman, "The Value System of Different Classes: A Social Psychological Contribution to the Analysis of Stratification," in *Class*,

Status, and Power . . ., pp. 426-441. For an example of different methods of communication used by various classes, see Carl I. Hovland, A. A. Lumsdaine, and F. D. Sheffield, "The Effect of Presenting 'One Side' versus 'Both Sides' in Changing Opinions on a Controversial Subject," in Wilbur Schramm, editor, *The Process and Effects of Mass Communication*, Urbana: University of Illinois Press, 1955, p. 267.

⁷ Peter M. Blau does not use these concepts, but in effect makes the same distinctions in "Social Mobility and Interpersonal Relations," *American Sociological Review*, 21 (June, 1956), pp. 290-295.

from deference by his working class neighbors.

Given this distinction, the extended family plays an especially important role for those who are upwardly mobile. Parents' energies are frequently directed toward having their children learn a trade or profession that has more prestige than their own.⁸ This is completely different from the situation in which occupational colleagues often look upon upward mobility as evidence of an *arriviste*. Significantly, the family visit can be isolated from friends' visits. As a consequence, a person can achieve status by gaining deference from his family and by associating with friends. This is possible because in American society there is an institutional basis for keeping friendship and family visits separated, which is further encouraged by the anonymity of very large cities.

The fact that size of city plays an important role in support of the family's status function casts some doubt on a body of literature (for example, Warner's "Yankee City" studies) which suggests that extended family relations are not consonant with occupational mobility. These studies are characterized by the fact that they were made in relatively small towns which cannot be considered representative of present or future urban society. Small towns do not provide anonymity and therefore may be much more likely to encourage family dissociation among the upwardly mobile because the individuals are forced under these circumstances to choose between status by deference and status by association.

In addition to fulfilling a status function for those who are upwardly mobile there is some evidence that the extended family provides status aid to those who are either downwardly mobile or in the initial stages of their career. This status aid is frequently isolated from occupational appointment and as such does not lead to the dangers of nepotism. Sussman gives an illustration of this situation: about 70 per cent of his 97 white mid-

dle-class Protestant New Haven parents said that they gave sufficient aid to married children to influence their status position, and that if the aid were withdrawn their status position would drop.⁹ What is significant about this aid is that it was rarely directed towards job advancement or appointment, but was concerned instead with the standard of living, for example, gifts for clothing, aid for house purchase, vacations, newborn babies, and during illness.¹⁰

⁹ Marvin B. Sussman, "The Help Pattern in the Middle Class Family," *American Sociological Review*, 18 (February, 1953), p. 27.

¹⁰ *Ibid.* pp. 22-28, *passim*. There are at least five mechanisms by which extended family structure is made consistent with the demands for occupational appointment by merit. For an analysis of these mechanisms see Eugene Litwak, *Primary Group Instruments for Social Control in Industrial Society: The Extended Family and the Neighborhood*, unpublished Ph.D. thesis, Columbia University, 1958. These may be briefly described as follows:

1. Norms of occupational appointment by merit can be inculcated even within the extended family structure.
2. Significant extended family aid can take place in non-occupational areas and therefore not all extended family aid need be contrary to the demands of occupational appointment by merit.
3. The professionalization of occupations has provided more objective measures of merit and therefore less reliance need be put on rules against nepotism.
4. Developed bureaucratization of occupation means that many jobs no longer belong to the individual and are therefore more difficult to hand down to relatives.
5. Rules against nepotism are costly in a mature industrial bureaucracy because they isolate the family from the occupation and play down its importance when the family in fact has a growing role in productivity. As Reinhard Bendix suggests in *Work and Authority in Industry*, New York: Wiley, 1956, pp. 226 ff., the industrial bureaucracy in its mature state must make use of interpersonal relations as technical aspects of the job. Leadership ability, capacity to communicate with others, an inner motivation to work, all become central when the work situation means dealing with other people rather than machines. Unlike classical job attributes (engineering knowledge, accounting knowledge, mechanical skills, etc.) it is difficult if not impossible to isolate interpersonal attributes from the non-work situation. The family plays a major role in the development and continuing use of these interpersonal abilities. The large organizations, in order to increase their productivity, have found it necessary to stress a happy family life and the identification of the family with organizational goals. See William H. Whyte, Jr., *Is Anyone Listening?* New York: Simon and Schuster, 1952, pp. 145 ff.

Any isolation of the family from the com-

⁸ For one of several studies which indicate that parents have high hopes that their children will do better than they have done, see Arthur Kornhauser, "Analysis of 'Class' Structure of Contemporary American Society: Psychological Bases of Class Divisions," in G. W. Hartman and T. Newcomb, editors, *Industrial Conflict*, New York: Cordon, 1939, pp. 240-241.

Because of such considerations it is hypothesized that the extended family can and will increasingly provide aid for the mobile nuclear family without interfering with its mobility or occupational efficiency. In turn, the extended family, in providing status to mobile people, is viable.¹¹

Differential socialization, occupational mobility, and extended family cohesion.—The second objection to the claim that the extended family is viable among occupationally mobile groups—namely, that mobility leads to differential socialization—may be answered if it is hypothesized that class differences are moderate or shrinking, but not growing larger.¹² This hypothesis is advanced

pany or any attempt to minimize the role of the family is likely to be costly to the organization since it means loss of control over a valued job skill. If it is true, as suggested in the four mechanisms above, that the dangers from the merger between family and industry have become minimized, and if it is true that the dangers of isolation have increased, then there is good reason to expect strong organizational support for family life as good *per se*. In this connection it would be argued that extended family relations derive benefits as well.

¹¹ It should be recognized that the status aid provided by the extended family is supplemental and in an absolute sense contributes far less than the occupation of the nuclear family. However, this family aid is of a psychologically crucial sort since it provides the nuclear family with the cutting edge of status advancement. As such, family support may be viewed as far more decisive by the family members than its absolute value would suggest. For an elaboration of this view see Eugene Litwak, "Geographic Mobility and Extended Family Cohesion," *American Sociological Review* (forthcoming).

¹² It is here assumed that the extended family has not lost its functions or narrowed its scope, as suggested, for example, by William F. Ogburn in "The Changing Functions of the Family," in R. F. Winch and R. McGinnis, editors, *Studies in Marriage and the Family*, New York: Holt, 1953, pp. 74-80. Rather what is assumed is that each area of life can be divided into those parts which have become standardized and uniform and those parts which are idiosyncratic. The development of large-scale bureaucratic organizations has meant a division of labor with the family handling the idiosyncratic and the large organization the uniform aspects of life. This is a function of the superiority of bureaucratic organizations for handling the uniform. Max Weber develops this point in *From Max Weber: Essays in Sociology*, H. H. Gerth and C. Wright Mills, translators and editors, New York: Oxford, 1946, pp. 196-244. Implicit in most of the criticism of Weber's formulation is the view that the large bureaucratic organization is not as well suited as the primary group for handling the idiosyncratic: it

because of the rise in educational standards, the development of mass media, the general rise in the standard of living, and the drop in large-scale immigration. Of course, there are *both* major class differences and similarities in contemporary American society.¹³ In this connection, what are the comparative

does not have the flexibility in committing its resources nor the short lines of communication which allow for speedy action. For a review of these criticisms see Peter M. Blau, *Bureaucracy in Modern Society*, New York: Random House, 1956, pp. 61-67. The extended family is a primary group and as such will have a continuing function in most areas of life, for in all areas there are idiosyncratic as well as uniform events. Several studies provide indirect substantiation that the extended family is viable in urban society: a study in Los Angeles by Scott Greer, "Urbanism Reconsidered," *American Sociological Review*, 21 (February, 1956), pp. 22 ff.; a study by Morris Axelrod, "Urban Structure and Social Participation," *Ibid.*, pp. 13-18; and a study in Buffalo, by Litwak, "Geographical Mobility and Extended Family Cohesion," *loc. cit.*, all reveal that close to 50 per cent of the respondents visited their relatives once a week or more. In addition, a fourth study in San Francisco by W. Bell and M. D. Boat, "Urban Neighborhoods and Informal Social Relations," *American Journal of Sociology*, 43 (January, 1957), pp. 391-398, shows that close to 90 per cent of the respondents name at least one relative as a close personal friend and 80 per cent say they can count on aid from their relatives if they are sick for a month or more. Finally, a study in New Haven by Sussman, *op. cit.*, pp. 27 ff., indicates that close to 70 per cent of the respondents said that if they withdrew their aid to their married children, they would suffer a significant drop in socio-economic status. All of these studies explicitly deal with middle class native Americans. In contrast, several investigations treat working-class or ethnic groups; but these also indicate certain strengths of extended family relations—see, e.g., M. Young and P. Willmot, *Family and Kinship in East London*, London: Routledge and Kegan Paul, 1957. However, these latter studies do not confirm the hypothesis presented in this paper, but rather support Parsons' hypothesis, for the most part indicating that extended family relations go hand in hand with occupational nepotism and geographical propinquity. Young and Willmot, in stressing the virtues of these classical extended family relations, do not consider their costs in terms of a democracy's industrial system. I believe that most ethnic-group and working-class studies that deal with the family are concerned with vestiges of the "classical extended" family, since the working-class groups are the last to feel the bureaucratic pressures discussed above regarding modified extended family relations. For a systematic discussion of this point, see Litwak, *Primary Group Instruments of Social Control* . . . , *op. cit.*, pp. 12 ff.

¹³ Cf. Kornhauser, *op. cit.*

magnitudes of the differences and similarities? And, are differences shrinking or growing larger over time? Class similarities seem to be sufficiently large to provide cross-class identification by extended family members and the differences appear to be shrinking.¹⁴ The issue of shrinking class differences should not be confused with the issue of class conflict. For it is possible that class conflict may well occur between two relatively articulate groups who understand each other all too well but differ over what constitutes a fair division of the spoils.¹⁵

Regarding the problem of differential socialization, it should be noted that many of the studies which support Parsons' hypothesis use as their respondents first and second generation members of minority ethnic groups.¹⁶ In this way, the problem of class differences is compounded by the problem of ethnic differences, often leading to an exaggeration of the former.¹⁷

¹⁴ Nelson N. Foote and Paul K. Hatt, "Social Mobility and Economic Advancement," *The American Economic Review*, 43 (May, 1953), show that the tendency since 1900 is for the entire occupational structure to become more "middle-class." For a summary of studies dealing with class differences over time, see S. M. Lipset, "American Society—Stability in the Midst of Change," unpublished paper read at the National Conference of Social Welfare, San Francisco, California, May, 1959, pp. 16–19. In this paper Lipset refers to unpublished studies as well as the following articles which indicate that class differences may be moderate or shrinking: G. Sjöberg, "Are Social Classes in America Becoming More Rigid?" *American Sociological Review*, 16 (December, 1951), pp. 775–83; S. Goldsmith et al., "Size Distribution of Income Since the Mid-Thirties," *The Review of Economics and Statistics*, 36 (February, 1954), pp. 26 ff.; Kurt Mayer, "Business Enterprise: Traditional Symbol of Opportunity," *British Journal of Sociology*, 4 (June, 1953), pp. 160–180.

¹⁵ This view tends to characterize the contemporary English Labor Party. It was also explicitly expressed by Karl Kautsky in "Terrorism and Communism," reprinted in *Man in Contemporary Society: A Source Book Prepared by the Contemporary Civilization Staff of Columbia College, Columbia University*, New York: Columbia University Press, 1956, II, pp. 549–550.

¹⁶ E.g., Warner and Lunt, *op. cit.*; Walter Firey, *Land Use in Central Boston*, Cambridge: Harvard University Press, 1947, pp. 111–191, 220–225.

¹⁷ Fred L. Strodbeck's "Family Interaction, Values, and Achievement," in David C. McClelland et al., *Talent and Society*, Princeton: Van Nostrand, 1958, pp. 135–195 *passim*, points out how members of an ethnic group with a traditional extended

family orientation, e.g., the Italian, are less likely to achieve success than those of an ethnic group whose values more closely match that of the dominant society, e.g., the Jewish.

It is maintained in this paper that among the white Americanized groups (and especially those in the middle-range occupations) upward mobility does not involve radical shifts in socialization and therefore does not constitute a real barrier to extended family communication.¹⁸

Occupational power and extended family cohesion.—If it is granted that the extended family has important functions in contemporary society¹⁹ and that bureaucratic pressures tend to stress the family as an end value, there should then be a positive relationship between extended family contacts and economic resources. And if it is assumed that generally persons in the stationary upper class have the greatest resources, those in the stationary manual classes the least resources, and the occupationally mobile stand between these two groups in this respect, the view presented above can be specified operationally as follows: The stationary upper class respondent will have the greatest number of extended family visits, the upwardly mobile the next greatest number, the downwardly mobile the next most, and the stationary manual the fewest number of visits.

This point of view differs from Parsons' hypothesis which argues a curvilinear relationship, with the mobile individuals having fewer extended family visits than the occupationally stable.

FINDINGS

Sample and operational definition of mobility.—In order to investigate empirically the alternative hypothesis, a secondary analysis was made of data gathered in Buffalo, New York, for a housing survey. The sample consisted of 920 white married women from the Buffalo urban area who had moved into a new home just prior to the time of the study (between June and October, 1952). They all

family orientation, e.g., the Italian, are less likely to achieve success than those of an ethnic group whose values more closely match that of the dominant society, e.g., the Jewish.

¹⁸ In this connection it should also be noted that most occupational shifts do not involve major changes in class position. See, e.g., S. M. Lipset and R. Bendix, "Social Mobility and Occupational Career Patterns II: Social Mobility," in Bendix and Lipset, *op. cit.*, pp. 455, 457.

¹⁹ See footnote 12 above.

had children 19 years or younger. Several kinds of sampling procedures were used which cast doubt upon the relevance of traditional statistical tests.²⁰ For purposes of the present discussion, however, the biases of the sampling are of great value. The population studied was predominantly middle class, and native born; as such, it is made up of persons who—according to Parsons—are most likely to bear out his hypothesis.²¹

Occupational mobility was ascertained by comparing the occupations of the husband and the husband's father. These occupations were first classified according to the Census Bureau's occupational scheme and were then divided into three overall classes as follows:

- | | |
|-------------------------|--|
| 1. <i>Upper class:</i> | Professional, technical and kindred
Managers, officials, and proprietors |
| 2. <i>Middle class:</i> | Clerical and kindred workers
Sales workers |
| 3. <i>Manual class:</i> | Craftsmen, foremen, and kindred workers
Operatives and kindred workers
Service |

per class whose parents were also in the upper class.

2. *Upwardly mobile class:* All those whose parents came from a lower occupational group than that which the husband now occupies.

3. *Downwardly mobile class:* All those whose parents came from a higher occupational group than the respondent now occupies.

4. *Stationary manual class:* All those who are now manual workers and whose fathers were manual workers.

Eliminated from the analysis were all respondents whose fathers were farmers and all persons in the middle class whose fathers were also in the middle class—the former because of difficulty in classifying them, and the latter since there were only thirty cases

Percentage of
Sample in Each Group
(husbands' occupations only)

25.6

24.2

3.8

10.3

21.6

12.5

1.4

100.0 (1000)

After assigning both the husband and his father to one of these classes, four mobility categories were defined:

1. *Stationary upper class:* All those now in the up-

²⁰ For a detailed description of the samples see Glenn H. Beyer, Thomas Mackesey, and James E. Montgomery, *Houses Are for People: A Study of Home Buyer Motivation*, Ithaca: Cornell Housing Research Center, 1955. The sample was stratified for income, owners-renters, and house design. In addition, some groups were selected on a block cluster basis, i.e., all people on the block with children under 19, while others were selected on an area sampling basis—within income and family cycle limitations. Because of the extremely varied nature of the sample it is difficult to know what constitutes a correct statistical test. Therefore the meaningfulness of the empirical data must rest to a considerable extent on the theoretical plausibility of the discussion and its consistency with other studies. However, if the assumptions of a random area sample could be made, then all major findings are significant at the .05 level and beyond. The sign test or the Wilcoxon matched pairs signed-ranks test was used. All tests were based on signs taken from the most complex tables in which the given variables appeared, i.e., Tables 4, 5, 6, and 8.

²¹ Parsons, "The Social Structure of the Family," *op. cit.*

(it was thought that they would only blur the analysis if they were arbitrarily classified with upper or manual groups).

Occupational mobility and extended family visits.—In order to ascertain the presence or absence of extended family relationships the respondents were asked, "How often do the following people come to your house: relatives who just drop in, relatives who are invited?" With certain noted exceptions, all individuals who reported one or more visits a week were classified as having frequent family visits. Using these definitions, initial empirical examination of the hypothesis of a negative relation between extended family contact and occupational mobility may be undertaken. Table 1 provides no support for this hypothesis: there is almost no difference between the mobile and non-mobile categories. If Table 1 does not support this hypothesis, neither does it support the alternative one suggested above. The lack of relationship, however, is due to the fact that geographic mobility has not been taken into account. Individuals who have no relatives in the city find it extremely difficult to visit.

TABLE 1. THE OCCUPATIONALLY MOBILE ARE AS LIKELY AS OTHER GROUPS TO HAVE FAMILY VISITS

	Percentage Receiving One or More Family Visits a Week
Stationary upper class	36 (249)*
Upwardly mobile class	34 (288)
Downwardly mobile class	37 (142)
Stationary manual class	39 (245)

* In this and the following tables, numbers in parentheses indicate population upon which the percentage is based.

Since a companion paper²² shows that families can maintain their extended family identification despite geographic distance, the present analysis attempts to exclude the geographic factor and to restrict the analysis to occupational mobility. This can be accomplished partially by analyzing separately those respondents with and without relatives living in the city. Table 2 shows that the negative relation between mobility and extended family contact is not substantiated even where geographic mobility is taken into account. Among the respondents with relatives residing in the city, those in the upper class have the most visits, those in the manual groups the fewest, and the mobile respondents an intermediate proportion. Among those with no nearby relatives, the stationary manual class shows the highest proportion of frequent family visits, while the other classes report a fairly similar number of visits.

The difference in patterns between those with and without nearby relatives seems to be accounted for by the crudeness of the measure used to determine how far the respondents live from their relatives. Considerable evidence suggests that the upper-class person who has no relatives in the city is likely to live at a much greater distance from them than the manual worker without nearby relatives. One Census report, for example, shows that professional groups are more likely than manual workers to move greater distances.²³ Again, data in this re-

TABLE 2. GEOGRAPHICAL RATHER THAN OCCUPATIONAL MOBILITY ACCOUNTS FOR LARGE DROP IN FAMILY VISITS

	Percentage Receiving One or More Family Visits a Week	
	Respondents Having Relatives in City	Respondents Not Having Relatives in City
Stationary upper class	59 (148)	2 (101)
Upwardly mobile class	51 (183)	5 (105)
Downwardly mobile class	51 (101)	0 (41)
Stationary manual class	43 (216)	10 (29)

search reveal that manual workers without relatives in the city are much more likely (21.5 per cent) to have been raised in the community under study than any other group (one per cent). In short, a lack of nearby relatives is not as apt to signify geographical mobility for manual workers as it is for other groups. Therefore such manual workers are more likely to have relatives in the immediate vicinity than other groups without nearby relatives. This probably accounts for the high proportion of manual workers without relatives in the city who have frequent family visits (as presented in Table 2).

Extreme occupational mobility and extended family visits.—The fact that the initial data do not verify the negative relation between mobility and extended family may well be accounted for by the measure of mobility, which was operationalized on an absolute rather than a relative basis. Foote and Hatt (among others) point out that the entire population has risen in occupational status during the last 60 years. As a consequence, respondents may have no psychological sense of mobility even though they are better off than their parents. To experience mobility, then, the individual must move up the social scale at a faster rate than his peers.²⁴ Perhaps mobility is negatively related to the extended family if relative rather than absolute mobility is taken into account.

To investigate this question only those mobile individuals who moved from stationary manual groups to upper class groups or

²² See footnote 11, above.

²³ U. S. Bureau of the Census, *Sixteenth Census of the United States: 1940 Population, Internal Migration 1935 to 1940, Economic Characteristics of Migrants*, Washington: U. S. Government Printing Office, 1946, p. 9.

²⁴ Foote and Hatt, *op. cit.*, pp. 370-376.

TABLE 3. THOSE MORE OCCUPATIONALLY MOBILE THAN THEIR PEERS HAVE AN INTERMEDIATE PROPORTION OF EXTENDED FAMILY VISITS

	Percentage Receiving One or More Visits a Week	
	Respondents Having Relatives in City	Respondents Not Having Relatives in City
Stationary upper class	59 (148)	2 (101)
Extreme upwardly mobile *	59 (114)	2 (58)
Extreme downwardly mobile *	50 (51)	0 (12)
Stationary manual class	43 (216)	10 (29)

* It is assumed that those who move from manual class to upper class or *vice versa* are more mobile than most people in the country.

vice versa were considered. This constitutes a two-step move and would presumably encompass all individuals who have moved faster than their peers. Table 3 indicates that the negative relationship does not appear even where extremely mobile people are taken into account. Among those with relatives in the city the mobile individuals show no lower proportion of frequent visits.

Age, occupational mobility, and extended family visits.—It is generally recognized that adequate analysis of occupational mobility requires consideration of age. For example, downward mobility means something different for persons at the beginning and at the middle of their careers. Because of the sampling procedure used in this study, more than 98 per cent of the respondents were 45 years old or younger, so that the factor of age can be examined only within a narrow limit.

The population was divided at the median age of the wife (29). Table 4 shows the irrelevancy of the age factor in this study: even when this factor is considered, the negative relation between mobility and extended family still is not substantiated—the pattern of family visits is not affected.

Occupational mobility and extended family identity.—Despite these findings it may be argued that family visits are not an adequate measure of family attachment and thus irrelevant to the criticism of Parsons' hypothesis. Occupationally mobile (especially upwardly mobile) persons may be in a position where family visits, although un-

TABLE 4. OCCUPATIONALLY MOBILE GROUPS HAVE AN INTERMEDIATE PROPORTION OF EXTENDED FAMILY VISITS EVEN WHEN AGE IS CONSIDERED

	Wife is 29* Wife is older or Younger than 29	
		Relatives of the Husband or Wife Living in the City
		Percentage Receiving One or More Family Visits a Week
Stationary upper class	61 (80)	57 (67)
Upwardly mobile class	50 (93)	52 (89)
Downwardly mobile class	47 (58)	58 (43)
Stationary manual class	44 (119)	37 (95)
		No Relatives Living in the City
		Percentage Receiving One or More Family Visits a Month*
Stationary upper class	10 (48)	18 (51)
Upwardly mobile class	18 (56)	27 (48)
Downwardly mobile class	12 (26)	0 (14)
Stationary manual class	36 (11)	33 (18)

* This figure was used because it is closest to the median age for the entire group.

* So as to have stable proportions, in the following tables a frequent visit for persons without relatives in the community is defined as above. Use of this definition does not alter the rank of any of the classes.

wanted, are thrust upon them. It may be maintained that the negative relationship hypothesis merely assumes that upwardly mobile individuals are least likely to be oriented toward extended families. If it can be demonstrated that this is the case—for example, that visits tend to be thrust upon such persons—the hypothesis in large part remains viable despite the negative results reported above.

In order to investigate extended family-orientation, respondents were asked to what degree they agreed with the following statements:

1. Generally I like the whole family to spend evenings together.
2. I want a house where family members can spend time together.
3. I want a location which would make it easy for relatives to get together.
4. I want a house with enough room for our parents to feel free to move in.

TABLE 5. OCCUPATIONALLY MOBILE GROUPS ARE MODERATELY IDENTIFIED WITH THEIR EXTENDED FAMILY

	Percentage Extended Family Orientation	Percentage Nuclear Family Orientation	Percentage Non-Family Orientation	Population
Stationary upper class	26	55	19	100 (247)
Upwardly mobile class	22	55	23	100 (284)
Downwardly mobile class	17	52	31	100 (147)
Stationary manual class	15	51	34	100 (242)

Those individuals who "very strongly agreed" with the third or fourth statement²⁵ were defined as extended family-oriented, those who agreed with the first or second statement as nuclear family-oriented, while those who rejected all of the statements were defined as non-family-oriented. These responses form a Guttman scale pattern, with the extended family-oriented tending to agree "very strongly" with at least the first three statements, the nuclear-oriented agreeing with the first two, and the non-family-oriented not agreeing "very strongly" with any of the statements.

The data shown in Table 5 suggest that the hypothesis under criticism does not hold even for extended family identification. Upwardly mobile individuals were not less identified with their extended family than the others. Rather, the degree of extended family-orientation increased as the respondents engaged more and more in upper class occupations. This finding is consistent with the view that bureaucratic occupations (mostly upper class²⁶) encourage the devel-

opment of the value of the family as an end value.

Extended family identification, status orientation, and mobility.—It could be maintained, however, that the analysis presented above does not really provide a precise test of the hypothesis that the extended family is negatively related to occupational mobility. For it could be inferred with some justification that this hypothesis only suggests that upwardly mobile persons will not be extended family-oriented only if they are status-oriented. Put somewhat differently, it could be argued that extended family-oriented individuals are unlikely to be status-oriented, especially if they are upwardly mobile.

Before investigating this point empirically, it should be noted that the term "status-oriented" is being used in the "high society" or "society news" sense. This is similar to Warner's use of "social class," and a little narrower than Weber's use of "status."²⁷

In order to operationalize status-orientation, the respondents were asked to answer two types of questions: those which express a concern for "social manners," and questions which stress the importance of "social" evaluations. Again, four questions were used, two of each type. Since there were no predetermined status positions similar to the family values (extended, nuclear, and non-

²⁵ It should be pointed out that because of the small number who answered statement 4 positively (four per cent) almost all of the extended family-oriented were defined by statement 3. For interrelations between the items see Litwak, *Primary Group Instruments of Social Control* . . . , *op. cit.*, pp. 43-47.

²⁶ In this paper and the companion paper cited in footnote 11 above, bureaucratic occupations refer to administrative and upper white-collar positions in bureaucratic organizations. Most of these people are members of the upper-class category, as a consequence of the definition of upper class used in this paper. If this category is to be used as an index of bureaucratic occupations, it must be known if those who are members of bureaucratic organizations form a sufficiently large group within the upper class to give it a distinctive bureaucratic style. Data from a study by Gold and Slater indicate that 74 per cent of their respondents who match the upper-class category in this study in terms of age and oc-

cupation are members of bureaucratic organizations. See Martin Gold and Carol Slater, "Office, Factory, Store—and Family: A Study of Integration Setting," *American Sociological Review*, 23 (February, 1958), pp. 66, 69. This would seem promising support for the assumptions made in this paper, but because of the sampling problems and the operational definitions used for bureaucratic organizations it cannot be conclusive.

²⁷ W. Lloyd Warner, Marchia Meeker, Kenneth Eells, *Social Class in America*, Chicago: Science Research Associates, 1949, pp. 3-46, *passim*; Weber, *op. cit.*, pp. 186-187.

TABLE 6. EXTENDED FAMILY ORIENTED PERSONS AND CONCERN WITH STATUS

	Percentage Who Express a High Concern for Status		
	Extended Family Orientation	Nuclear Family Orientation	Non-Family Orientation
Stationary upper class	70 (64)	47 (136)	31 (48)
Upwardly mobile class	76 (62)	49 (157)	33 (70)
Downwardly mobile class	79 (26)	47 (75)	37 (41)
Stationary manual class	64 (50)	39 (113)	34 (80)

family oriented) the population was divided in half to facilitate analysis.

The individual who answered positively any three of the questions listed below is considered, as a result of this procedure, to be status-oriented, while the respondent who answered only one or rejected all of them is described as having little concern for status. The following questions were asked:

1. When in public people should be extra careful of their behavior.
2. I'm uncomfortable when I am with people who have bad manners.
3. I want a house which I can be proud to have my friends see.
4. I think my house has a lot to do with my friends' opinion of me.²⁸

Assuming that these questions are adequate indicators, the data presented in Table 6 show that extended family-oriented persons are more likely than those in any other category to be status-oriented. Furthermore, this association holds for the upwardly mobile. Once more, these empirical findings are inconsistent with the hypothesis that mobility and family nucleation are mutually supporting.

Extended family relations and occupational power.—Thus far major emphasis has been given to showing that the negative relation between occupational mobility and extended family relations does not hold under successively more specified conditions. At this point an attempt is made to demonstrate positively the validity of the alternative hypothesis advanced here. It should first be noted that the data presented in the foregoing tables indicate that extended family visits and identification increase with occupational position or occupational re-

sources. The only exception is among those with no relatives living in the community, and this seems to be an understandable function of geographical mobility.

If, as hypothesized, occupational resources are a major element in extended family visits, there should be some distinctive differences in visiting patterns between persons with large and those with limited occupational resources. Among the former, frequent family visits are most likely to be part of a social context involving other forms of social participation since their economic resources permit a wide range of social activities. In contrast, among those individuals with limited resources family visits are likely to be isolated from other types of visits. This view is substantiated by the data presented in Table 7. Upper class persons are more likely than any others to have family visits in conjunction with neighborhood friendships and affiliation with voluntary associations. The manual class is more likely than any other class to have only family visits with minimal participation in neighborhood or voluntary associations. This combined with previous findings highlights the major role played by occupational resources in preserving extended family contact.

Joint interaction of family orientation, status orientation, occupational mobility, and extended family relations.—The several factors which have been examined independently can now be brought together. If these factors are considered simultaneously, the negative relationship hypothesis can be restated as follows: Individuals who are least likely to have extended family visits are (a) upwardly mobile, (b) status oriented, and (c) nuclear or non-family oriented. In contrast, the hypothesis suggested in this paper can be elaborated: Individuals who are least likely to have family visits are (a) characterized by fewer occupational resources (sta-

²⁸ For the interrelations between the items, see Litwak, *Primary Group Instruments of Social Control* . . . , op. cit., pp. 47-50.

TABLE 7. ECONOMIC POWER PERMITS BOTH FAMILIAL AND OTHER ACTIVITIES^a

	Percentage Participating in Family, Club, and Neighborhood Friendships ^b	Percentage Participating in Family and Either Club or Neighborhood Friendships ^b	Percentage Participating Only in Family Visits and No Other Activities ^b	Total Percentage Participating in Family Visits ^c
Stationary upper class	29	23	7	59 (148)
Upwardly mobile class	16	22	13	51 (183)
Downwardly mobile class	13	26	12	51 (101)
Stationary manual class	8	20	14	43 (216)

^a This table includes only those respondents who have relatives in the city because only under such conditions can it be assumed that relatives are at an equal distance from all occupational groups.

^b To participate in family activities means to receive family visits once a week or more; to participate in clubs signifies belonging to one or more (the closest approximation to the median number) clubs; to participate in neighborhood friendships means to know five or more (the closest approximation to the median number) neighbors well enough to call on them.

^c Total family visits means all people whether the visit is part of a pattern of participation or a single family participation.

tionary manual workers), (b) non-status oriented, and (c) nuclear or non-family oriented.

These hypotheses in part overlap and in part are contradictory. Table 8 permits the reader to see more clearly at what points the hypotheses run counter to one another. This table shows twelve "groups" enclosed within a dotted line. According to the view that the extended family is negatively related to mobility, two of these groups should have a lower proportion of frequent family visits than the other ten—consisting of those who are either nuclear or non-family oriented, concerned with status, and who are upwardly mobile. The hypothesis advanced in this paper leads to a different prediction: that the two groups which will have the lowest proportion of frequent family visits are nuclear or non-family oriented but *not* status oriented and, in addition, come from the *non-mobile* manual category. (Neither hypothesis can be tested by the behavior of the groups in the cells lying outside the dotted line, for both hypotheses would predict that these would have a higher proportion of frequent visits than all others.)

If the cells within the dotted lines are contrasted, then in only three out of ten possible comparisons is the negative relationship hypothesis supported, while the counter hypothesis is supported in seven out of ten instances. This may be seen more clearly if the cells are grouped into (1) those which according to the hypothesis will be

the lowest, and (2) all others.²⁹ If this is done, then the group which would be expected to have the lowest proportion of frequent visits if there were a negative relationship between mobility and extended family, show in fact the highest proportion (54 per cent as compared to 34 per cent for the rest of the population). In contrast, the group which according to the counter-hypothesis would be expected to have the lowest proportion of family visits actually reveals this pattern (30 per cent as compared to 54 per cent for the total population). In short, when the negative relationship hypothesis is specified in its most complete form it is not substantiated, while the alternative hypothesis tends to be.

DISCUSSION AND CONCLUSION

It seems quite probable that Parsons' hypothesis is applicable in the earlier stages of industrialization. In that period extended family relations were heavily influenced by the peasant farm family, which strongly in-

²⁹ The two "groups" which, according to Parsons' hypothesis, should contain on the average the lowest proportion of frequent family visits had respectively 55 (49) and 40 (15) per cent with frequent family visits. In contrast, the two groups suggested by the counter-hypothesis to have the lowest proportion of frequent family visits had respectively 36 (67) and 27 (52) per cent with frequent family visits. The other groups within the dotted line were averaged together and added to either of the two averages above, depending on which hypothesis was being tested.

TABLE 8. UPWARDLY MOBILE, STATUS, AND NON-EXTENDED FAMILY ORIENTED RESPONDENTS HAVE AN INTERMEDIATE PROPORTION OF FREQUENT FAMILY VISITS^a

	Percentage Receiving One or More Family Visits a Week					
	Extended Family Oriented		Nuclear Family Oriented		Non-Family Oriented	
	High Status Concern	Low Status Concern	High Status Concern	Low Status Concern	High Status Concern	Low Status Concern
Stationary upper class	63 ^b	49	74	55	56	47
Upwardly mobile class	61	75	55 ^c	43	40 ^c	41
Downwardly mobile class	77	33	33	64	16	29
Stationary manual class	73	60	22	36 ^d	56	27 ^d
Population						
Stationary upper class	35	10	35	40	9	19
Upwardly mobile class	28	8	49	51	15	32
Downwardly mobile class	13	3	24	28	13	20
Stationary manual class	26	5	41	67	23	52

^a In this table, as in Table 7, only respondents are considered who have relatives living in the city. This avoids the extraneous consideration of geographical distance.

^b This cell should be read as follows: 63 per cent of the 35 people who had relatives living in the city were extended family oriented, concerned with status, stationary upper class, and had visits from relatives once a week or more.

^c According to Parsons' hypothesis, these two groups should have the lowest proportion of frequent family visits.

^d According to the alternative hypothesis, these two groups should have the lowest proportion of frequent family visits.

corporated the values of geographic and occupational closeness. Under such circumstances extended family relations were detrimental to occupational mobility.

In contemporary society, however, extended family relations develop from different institutional sources and as a result do not rely on geographic and occupational proximity for their viability.³⁰ It is suggested

³⁰ In comparing Italian and Jewish families in contemporary society, Strodbeck, *loc. cit.*, discusses shifts in extended family relations. The Italian family structure, relatively speaking, is still defined in terms of geographical and occupational proximity. In contrast, the Jewish extended family, for historical reasons, closely resembles the family structure considered to be prototypical of contemporary urban bureaucratic life. Thus Jewish families, more than Italian, encourage mobility. Strodbeck's interpretation of Jewish family structure seems to be largely an application of Parsons' hypothesis. Contrastingly, it is argued here that Strodbeck's study merely establishes the fact that the Jewish extended family does not define geographical and occupational mobility as an essential

that these modified extended family relations are more consonant with occupational mobility than the isolated nuclear family.

Several studies which seem to substantiate the view that the extended family is negatively related to occupational mobility suffer from the following defects: (1) they deal with small town populations which are not typical of urban development;³¹ (2) they focus upon ethnic and working class groups with their passing remnants of classical extended family relations rather than developing modified extended family relations;³² and (3) they frequently present anecdotal

process, nor is the nuclear family defined as subordinate to the parental family. Strodbeck neither demonstrates that the Jewish family is an isolated nuclear family nor that it does not consist of a series of nuclear families with strong equalitarian bonds through which pass much extended family aid.

³¹ Warner and Lunt, *op. cit.*

³² Firey, *op. cit.*; Young and Willmot, *op. cit.*

reports rather than systematic evidence, and therefore are more likely to concentrate on sensational family disruptions than ordinary uniformities.³³ Aside from the shortcomings of these earlier studies, there are some investigations which provide positive support for the hypothesis suggested in this paper.³⁴

Yet there are some major limitations in the present inquiry which must be dealt with before the findings can be fully accepted. A more adequate sample is necessary than the present one, in which individuals on the bottom of the occupational ladder—unskilled laborers—are not adequately represented. It could well be argued that upward mobility from this group would provide evidence supporting Parsons' hypothesis. However, it should be noted that the proportion of the labor force in this category has greatly declined during the last fifty years and that unskilled workers are likely to disappear as a significant factor in urban life under the twin pressures of automation and bureaucratization. This study is also limited by the fact that there are no data on people over 45 or those without children; there might be sig-

nificant differences within these two groups.³⁵ Even more important, additional work should be done on the indexes of extended family relation, occupational mobility, and the differentiation between bureaucratic and non-bureaucratic occupations if the hypothesis suggested in this paper is to be substantiated.

Despite these limitations, it is concluded that the hypothesis of a negative relation between occupational mobility and the extended family is not sufficiently established to support future research by itself. That part of Parsons' hypothesis which points to the functional inadequacy of the classical extended family is accepted, but that part which posits the isolated nuclear family as the most functional type for contemporary industrial society is rejected. It is hoped that the present modification and elaboration of Parsons' formulations will provide some additional understanding or raise significant questions about the relation between the family and the occupational system.³⁶

³³ Litwak, "Geographical Mobility and Extended Family Mobility," *loc. cit.*

³⁶ Litwak, *Primary Group Instruments of Social Control* . . . , *op. cit.*, pp. 16-37. Some of the basic underlying assumptions of bureaucracy are re-examined in terms of the problems posed here.

³³ Warner and Lunt, *op. cit.*

³⁴ See footnote 12 above.

ILLEGITIMACY IN THE CARIBBEAN SOCIAL STRUCTURE *

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Just as the family system is the keystone of any stratification system, so is legitimacy the keystone of the family system. A study of variations in norms and practices related to legitimacy should therefore illuminate the operation of social systems. The present paper deals with illegitimacy patterns primarily in the Caribbean, with the aim of throwing light on the following problems: (1) the correctness of Malinowski's Principle of Legitimacy; (2) the class distribution of a norm; (3) the conditions under which an important norm may be accepted, but violated in practice more often than not.

OVER a generation ago Malinowski enunciated a principle which he said amounted to a universal sociological law, that "no child should be brought into the world without a man—and one man at that—assuming the role of sociological fa-

ther. . . .¹ This rule is not based on the social disapproval of premarital or extramarital sexual freedom. Malinowski's Trobrianders, for example, indulged in considerable sex play before marriage, but were shocked at

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¹ Bronislaw Malinowski, "Parenthood, the Basis of Social Structure," in V. F. Calverton and Samuel D. Schmalhausen, editors, *The New Generation*, New York: Macaulay, 1930, pp. 137-138.

illegitimacy. Rather, the rule expresses the interest of the society in fixing responsibility for the child upon a specific individual. Marriage, therefore, is not primarily the legitimation of sex, but the legitimation of parenthood.² Whether Malinowski's principle is indeed a universal sociological law has not been analyzed, except to the degree that the recurring debate as to whether the "nuclear family" is universal implicitly includes that principle.³ It seems safe enough to claim at least that all societies have family systems and that possibly a sociological father is required everywhere.⁴

ILLEGITIMACY IN THE CARIBBEAN

Malinowski's principle is not refuted by data from the United States or Western Europe, where illegitimacy rates range from perhaps four or five per cent to about eleven per cent.⁵ However, in the Caribbean area

² Malinowski was puzzled as to how the Trobrianders could be sexually so free without numerous illegitimacies, especially since they denied any connection between sexual intercourse and pregnancy and took no contraceptive precautions. It was not until W. F. Ashley-Montagu's *Coming Into Being Among the Australian Aborigines*, London: Routledge, 1937, that the solution seemed to be clear.

See also M. F. Ashley-Montagu, *The Reproductive Development of the Female*, New York: Julian, 1957.

³ For a recent discussion of this point, see Melford E. Spiro, "Is the Family Universal?" *American Anthropologist*, 56 (October, 1954), pp. 839-846.

⁴ The most notable case which raises doubts is the Nayar of Malabar Strait. See K. M. Panikkar, "Some Aspects of Nayar Life," *Journal of the Royal Anthropological Institute*, 48 (July-December, 1918), esp. pp. 260 ff; E. Kathleen Gough, "Changing Kinship Usages in the Setting of Political and Economic Change among the Nayar of Malabar," *Journal of the Royal Anthropological Institute*, 81 (Parts I and II, 1951), pp. 71-88. Gough's latest report ("The Nayars and the Definition of Marriage," *Journal of the Royal Anthropological Institute*, 89 [1959], p. 31) asserts that Nayar marriage does establish paternity legally. Another possible case is the Minang-Kabau; see E. N. Loeb, "Patrilineal and Matrilineal Organization in Sumatra, Part 2," *American Anthropologist*, 36 (January-March, 1934), pp. 26-56.

⁵ In Ireland, illegitimate births constituted 27.9 per cent of live births in 1950, and the rate in Stockholm and a few other areas in Sweden has remained at about 15 per cent in recent years. Cf. Meyer Nimkoff, "Illegitimacy," *Encyclopedia Britannica*, 1954.

TABLE 1. ILLEGITIMACY RATES IN SELECTED CARIBBEAN POLITICAL UNITS⁶

Political Unit	Year	Per Cent
British Guiana	1955	35
French Guiana	1956	65
Surinam (excluding Bush Negroes and aborigines)	1953	34
Barbados	1957	70
Bermuda	1957	30
Dominican Republic	1957	61
Guadeloupe	1956	42
Jamaica	1954	72
Antigua	1957	65
Martinique	1956	48
Trinidad and Tobago	1956	47
Grenada	1957	71
Puerto Rico	1955	28
Haiti	—	67-85

illegitimacy rates are often over fifty per cent, as Table 1 shows.

Under such conditions, doubt may be raised as to whether a "sociological father" exists, and indeed various writers have spoken of a "matri-focal family."⁷ Certainly

⁶ All figures except those for Puerto Rico, British Guiana, Surinam, Dominican Republic, Trinidad and Tobago, Grenada, and Haiti were taken from the United Nations Year Book Questionnaire for the years in question. Data were furnished to the U.N. by the statistical offices of the country, and contain all the errors of their own registration procedures. Data for other countries, excluding Haiti and the Dominican Republic, were kindly furnished by the Caribbean Commission. The Dirección General de Estadística of the Dominican Republic graciously sent me the figure for 1957. I have found no recent figure for Cuba; presumably it was 30 per cent in 1939. For Surinam, Rudolf van Lier, *Samenvatting in een Gransgebied*, 's-Gravenhage: Martinus Nijhoff, 1949, p. 287, gives 70 per cent for 1940. The rate has also dropped in British Guiana from the 41 per cent reported in 1946 in *British Guiana Annual Report of the Registrar-General, 1954*, Georgetown, Demerara, British Guiana, 1956, p. 9. I have found no official figure for Haiti. Bastien reports two-thirds for Marbial (Remy Bastien, *La Famille Rurale Haïtienne*, Mexico: Libra, 1951, p. 85); George E. Simpson reports about 85 per cent for one Haitian area in "Sexual and Family Institutions in Northern Haiti," *American Anthropologist*, 44 (October-December, 1942), p. 664.

⁷ "One of the regularities of social organization, which has appeared in the literature from Herskovits to Henriques, is the concept of the 'matri-focal' family." Vera Rubin, "Cultural Perspectives in Caribbean Research," in Vera Rubin, editor, *Caribbean Studies: A Symposium*, Jamaica: Institute of Social and Economic Research, 1957, p. 117. Such comments are often applicable as well to one period in the development of the Negro family in

so high a rate of deviation would suggest that the norm, if it does exist, might have a very different meaning than in a society in which the rate is less, say, than ten per cent. But we must keep in mind that Malinowski was stating a proposition about a *cultural* element: he asserted that the *norm* would always be found, not that the members of the society would obey it under specified conditions.

It is precisely with reference to Malinowski's principle that many students of the Caribbean have taken an opposing position—without developing its implications for family theory. The claim has often been made for various Caribbean lands that when a couple is living together in a consensual union "the family may be said to exist in much the same way as it does in peasant communities throughout the world,"⁸ and the child therefore suffers no disadvantage from being illegitimate.⁹ Henriques, also writing about Jamaica, comments that there is no moral sanction against "concubinage," by which he means a man and woman keeping house together and raising children, and even claims that respectable black people would rather have their daughter become mistress or concubine to a white or fair colored man than marry a black one.¹⁰ Otherwise put, the consensual union is the marriage form of the lower classes in the Caribbean, and is "sociologically as legitimate" as a legal union. It is, in short, a "cultural alternative," as permissible a way of founding a family as any other.¹¹ If this interpretation is correct, Malinowski's principle would be erroneous, and one of the apparently major functions of the father would have to be redefined as unessential.

Comments similar to those given above

about Jamaica have been made about other Caribbean areas. Herskovits and Herskovits make a similar claim for Trinidad, noting what a "false perspective on the thinking of the people is given by the application of legal terms such as 'legitimate' and 'illegitimate' to the offspring."¹² Similarly, they assert that "there is no social disability imposed by the community because of legitimacy or illegitimacy."¹³ The common-law marriage is for many the accepted form.¹⁴

With respect to Haitian children of a *placée* union which is legalized, of a legal union, or of a union outside of an existing marriage the claim is made that "none of these classes of children are at any special social disadvantage."¹⁵ With reference to the forms of Haitian unions: "In the main, especially in the countryside, socially sanctioned matings which do not enjoy the approval of the Church endure as long and hold as respected a place in the community. . . ." ¹⁶ In a parallel vein, Bastien remarks that when a man has "good intentions" with respect to a girl, but does not have enough money with which to marry, he may "establish himself" with the girl, with marriage as a publicly acknowledged, later goal, but does not thereby "incur the scorn of the community."¹⁷

In Martinique, we are told, in place of the rule of legitimacy, which is absent here, other values have emerged such as ingroup solidarity, status equality, and conviviality, which express family organization.¹⁸ There is "no unequivocally preferred type of bond between parents."¹⁹ The legitimate and illegitimate share the same status.

Although the illegitimacy rate in Puerto Rico is lower than in the areas noted above,

this country. Cf. E. Franklin Frazier, *The Negro Family in the United States*, New York: Dryden, revised edition, 1948, Part 2, "In the House of the Mother."

⁸ T. S. Simey, *Welfare and Planning in the West Indies*, Oxford: Clarendon Press, 1956, p. 15.

⁹ "The fact of illegitimate birth is one completely taken for granted. An illegitimate child does not consider himself disadvantaged. . . ." *Ibid.*, p. 88.

¹⁰ Fernando Henriques, *Family and Colour in Jamaica*, London: Eyre and Spottiswoode, 1953, pp. 87, 90.

¹¹ "Thus, the matrifocal family . . . is a sub-cultural norm. . . ." John V. Murra, "Discussion," in *Caribbean Studies*, *op. cit.*, p. 76.

¹² Melville J. Herskovits and Frances S. Herskovits, *Trinidad Village*, New York: Knopf, 1947, p. 17.

¹³ *Ibid.*, pp. 82-83; see also p. 107.

¹⁴ Lloyd Braithwaite, "Social Stratification in Trinidad," *Social and Economic Studies*, 2 (October, 1953), p. 125.

¹⁵ Melville J. Herskovits, *Life in a Haitian Valley*, New York: Knopf, 1937, p. 118.

¹⁶ *Ibid.*, p. 106.

¹⁷ Bastien, *op. cit.*, pp. 72-73. However, Bastien also presents the prestige rankings of the three forms of matings.

¹⁸ Mariam Kreiselman, *The Caribbean Family. A Case Study in Martinique*, Columbia University, Ph.D. thesis, 1958, pp. 271, 292.

¹⁹ *Ibid.*, p. viii.

here too the claim has been made that the rule of legitimacy fails. It is said of the consensual union that it is "a cultural alternative," that is, marriage is split into two culturally permissible alternatives.²⁰ Similarly, "... the prevalence of consensual unions ought to be considered in terms of local lower-class conceptions of what is considered 'moral.' " It is not that the lower class prefer illegal behavior, but that consensual unions are not seen as immoral.²¹ It is asserted, too, that "the consensual union is considered a binding marriage truly cemented at the birth of the first child."²²

At first glance, then, Malinowski's rule of legitimacy is refuted. A substantial number of societies in the West appear not to accept the norm. If this is the case, then several fundamental notions in family theory would have to be discarded.

Yet a closer examination of these and other reports prove conclusively that the norm exists, since in fact marriage is the ideal, and those who violate the rule do suffer penalties. The fact that perhaps a majority of certain of these populations do live in unions outside marriage, at some time in their lives, does not change the normative status of the rule. On the other hand, as we shall later indicate, Malinowski's rule must nevertheless be reformulated.

Let us first look more closely at Jamaica. As against the assertion that illegitimacy is not stigmatized, we note the opposing facts. Both upper and middle class opinion is set against "concubinage."²³ The priests may shame the couple about the matter. When a young girl is found to be pregnant, her family is angry.²⁴ Few men (Rocky Roads) allow their women to bring their illegitimate

TABLE 2. BRITISH WEST INDIES: PER CENT OF MALES EVER MARRIED BY AGE, 1946

Age	Jamaica	Barbados	Windwards	Leeward
20-24	10.1	8.5	4.1	4.0
25-34	21.0	37.6	27.6	27.3
35-44	41.9	61.7	54.9	53.4
45-54	55.0	70.8	68.2	63.7
55-64	66.3	75.2	78.4	75.5
65 and over	74.7	85.2	83.1	78.9

children into the union, if they do marry.²⁵ In the same community, illegitimate children are subjected to more physical rejection and pressures of sibling rivalry.²⁶ Moreover, as individuals move through the life cycle, an increasing proportion are actually married, a phenomenon which would be inexplicable if the consensual unions were backed by a set of alternative norms. This process is illustrated by the proportions of persons ever married by selected ages in the major areas of the British West Indies, as shown in Table 2.²⁷ Thus, though the average British West Indian ages at marriage are among the highest in the world (for example, for Jamaica, 34.1 years for males; for Barbados, 31.7, and for Grenada, 33.0²⁸), most individuals do marry. In sum, these various mating forms are "not regarded as alternative forms of conjugal associations between which any individual was free to choose."²⁹

²⁵ Yehudi Cohen, "Structure and Function: Family Organization and Socialization in a Jamaican Community," *American Anthropologist*, 58 (August, 1956), p. 669.

²⁶ *Ibid.*, p. 672.

²⁷ G. W. Roberts, "Some Aspects of Mating and Fertility in the West Indies," *Population Studies*, 8 (March, 1955), p. 223. The figures for Jamaica in Table 2 presumably refer to 1943.

²⁸ *Ibid.*, p. 205. More fundamental data are the actual expressions of norms and ideals, to be found in Judith Blake's *Family Structure: The Social Context of Reproduction*, Ph.D. thesis, Columbia University, 1959, a study of the lower class Jamaican family. It is the first detailed investigation of the mechanisms through which the norms lose much of their coercive power. For a preliminary report from this study, see Blake, "Family Instability and Reproductive Behavior in Jamaica," *Current Research in Human Fertility*, New York: Milbank Fund, 1955, pp. 24-41.

²⁹ Clarke, *op. cit.*, pp. 77-78. It is significant that Clarke and Blake, who appear to be the only investigators to take seriously the Jamaican's own normative statements, assert unequivocally the normative underpinnings of a legal marriage.

²⁰ J. Mayone Stycos, *Family and Fertility in Puerto Rico*, New York: Columbia University Press, 1955, p. 110. I assume here the meaning of "culturally equivalent" or "normatively equal."

²¹ Sidney W. Mintz, "Cañamelar, The Subculture of a Rural Sugar Plantation Proletariat," in Julian Steward et al., *The People of Puerto Rico*, Urbana: University of Illinois Press, 1956, p. 377.

²² Robert A. Manners, "Tabara: Subcultures of a Tobacco and Mixed Crop Municipality," in *The People of Puerto Rico*, *op. cit.*, p. 144.

²³ Henriques, *op. cit.*, pp. 87, 164.

²⁴ *Ibid.*, p. 88. See also Edith Clarke, *My Mother Who Fathered Me*, London: Allen & Unwin, 1957, p. 99; and Kreiselman, *op. cit.*, p. 189.

Similarly, in Trinidad, a couple may finally marry after living together for some time, "for the position it gives the family." Among other things, "marriage is . . . a prestige phenomenon in terms of social or religious values." Though a couple will usually begin life together as "keepers," "such an episode is outside correct procedure." The unmarried keeper woman wears no ring, and only the married woman is called *Madam*.³⁰ Many people who rise in class find that their new rank is incompatible with the type of union they once entered. Moreover, when working-class women quarrel, one may point out that the other is not properly married.³¹

Although the case of Haiti seems more complex, the same conclusion seems inescapable. The prestige from the legal, Church union is of sufficient significance to "motivate weddings at which the children and even children of the principals act as attendants."³² The legal union cannot be broken as easily as the *plaçage*. When the unmarried girl becomes pregnant, she is beaten.³³ The woman in a *placée* union cannot demand as much from her man, and her children have no right to the name of the father.³⁴ Most persons would prefer to marry, and this is especially true of women.³⁵ Contemporary pressures are increasing the proportion who marry, but some gradations of prestige remain.³⁶ The *plaçage* is not stable: "perhaps three-fourths of the peasant men, and possibly more, have or have had at one time one or more mates in addition to a legal wife or *femme caille*."³⁷ "The consciousness of

their social inferiority so troubles . . . [them] . . . that few resist the temptation to explain the cause of their situation. . . ."³⁸

In Martinique, too, parents are angry at the pregnancy of the unmarried girl, who may have to leave her home. When talking about the consensual relationships of others, the term "concubine" is used. Many men will promise marriage, but deceive the girl. In a few reported cases of girls having babies, the parents pretended that the children were their own.³⁹ The consensual union is easily dissolved, and no social obligations are incurred by entering it.⁴⁰

Perhaps more conclusive for Martinique is an important finding, which grew out of an effort to understand the *fête*. In possibly every study of illegitimacy in the Caribbean, people are described as saying—most researchers have accepted this assertion—that they cannot marry because they cannot afford the wedding feast, without which the ceremony is a mockery. The couple will be laughed at later. The "cost of the wedding" is not the church expenses; in every country the Catholic Church (or others, where they are important) has offered nearly free weddings—but with rare acceptance. A few observers have doubted that the expense of the *fête* was the crucial item, even though it is substantial, emphasizing rather that the *fête* is an expression of community solidarity, a *rite de passage*, and a community validation of the union. Kreiselman is unique among observers in offering and, within limits, testing the hypothesis that most persons who can afford to live *en ménage* can also afford a *fête* and therefore a marriage, but that most who do not marry early or later do not have the same rank.⁴¹

Whether the rank differences among people of a lower stratum are so crucial, and whether a broad sample of stable consensual unions would show that it is mainly those of equal status who marry, remains to be seen.

ordinary *plaçage*, some forms of which involve several women living apart from one another.

³⁰ Bastien, *op. cit.*, p. 73.

³¹ Kreiselman, *op. cit.*, pp. 189, 223, 201, 191, 188.

⁴⁰ *Ibid.*, p. 231. All unions involve social obligations, of course, but the fact that the investigator makes this observation underlines the lack of community support for this type of union.

⁴¹ *Ibid.*, pp. 221-231. After a long consensual union, may marriage occur because the man and woman come to have the same rank?

³⁰ Herskovits and Herskovits, *op. cit.*, pp. 82, 84, 87, 93-94.

³¹ Lloyd Braithwaite, "Social Stratification in Trinidad," *Social and Economic Studies*, 2 (October, 1953), p. 125, 126.

³² Herskovits, *op. cit.*, pp. 106, 107.

³³ *Ibid.*, p. 110; George Eaton Simpson, "Sexual and Familial Institutions in Northern Haiti," *American Anthropologist*, 44 (October-December, 1942), p. 665.

³⁴ Herskovits, *op. cit.*, pp. 116, 119.

³⁵ Simpson, *op. cit.*, pp. 655, 658.

³⁶ Rhoda Metraux, *Kith and Kin*, Ph.D. thesis, Columbia University, 1951, pp. 197, 205-209.

³⁷ Simpson, *op. cit.*, p. 656. The *femme caille* shares her consort's house. Bastien, *op. cit.*, p. 73, gives three main categories of unions, in order of social rank: (1) marriage; (2) a union established with the idea of later marriage; and (3) the

But if this is the case even in Capesterre (Martinique), the relationship shows that the rule of legitimacy holds there. For the rule has as a major function the prevention of unions between wrong lineages, and in nearly every society the rules of marriage serve to confine legal unions mainly to men and women of equal rank.⁴²

In Puerto Rico, there is social disapproval of the consensual union, even though the sanction does not necessarily lead to conformity. Fathers become angry when their daughters elope, and almost everyone pays "lip service" to the superiority of marriage.⁴³ People may say that they get married in order to baptize the children.⁴⁴ Girls have "idealized feelings" about marriage ceremonies,⁴⁵ and often the girls' parents request or insist upon legal unions.⁴⁶

Two-thirds of both men and women in a national sample of Puerto Rico said that a consensual union is a bad life for a man, and over 80 per cent of the respondents made the same assertion for women.⁴⁷ Perhaps a more penetrating test of the normative status of the consensual union may be found in the attitudes expressed about a daughter entering a consensual union: only 7.4 per cent of the men and 5.5 per cent of the women admitted that this arrangement would either be "all right" or that "it's up to her; doesn't matter."⁴⁸

⁴² Perhaps the Natchez were an exception. See Kingsley Davis, "Intermarriage in Caste Societies," *American Anthropologist*, 43 (July-September, 1941), pp. 382 ff. Of course, a "free courtship system" achieves the same end; and one may date a person whom one may not marry without censure.

⁴³ Stycos, *op. cit.*, pp. 108, 110-111.

⁴⁴ Eric R. Wolf, "San José: Subcultures of a 'Traditional' Coffee Municipality," in *The People of Puerto Rico*, *op. cit.*, p. 220. In Puerto Rico, the girl is usually a virgin when she enters a consensual union. Bastien makes the same claim for the Marbial area in Haiti, but to my knowledge no observer of other Haitian areas has done so; and Bastien is inconsistent. See Bastien, *op. cit.*, pp. 64, 65, 72.

⁴⁵ Mintz, *op. cit.*, p. 378.

⁴⁶ Elena Padilla Seda, "Nocora: The Subculture of Workers on a Government-Owned Sugar Plantation," in *The People of Puerto Rico*, *op. cit.*, p. 203.

⁴⁷ Paul K. Hatt, *Backgrounds of Human Fertility in Puerto Rico*, Princeton: Princeton University Press, 1952, p. 127.

⁴⁸ *Ibid.*, p. 64. I would suppose, however, that the percentage would be much less on the mainland of the United States.

We are similarly told that in British Guiana the children born outside wedlock "are not sharply differentiated by any stigma of illegitimacy," while the consensual union is a "socially sanctioned one," and "part of the lower-class tradition."⁴⁹ Once again, however, we can note that parents are angry at the daughter and beat her when she becomes pregnant while still in the home. An unmarried mother will usually ask another person to take her illegitimate child to church for baptism.⁵⁰ And, although the scholar here quoted agrees with a turn-of-the-century French writer on the Congo who asserted that among the Bavili "birth sanctifies the child," a man's "outside" children in British Guiana do not rank equally with his legitimate children, and not all of a woman's children remain with her in a new marital union.⁵¹ Moreover, only the married woman is called "Mistress," while her marital rights are clearer and more secure.⁵² Marriage confers a different status on the woman. Women wish to marry, and after they have begun to have illegitimate children they understand that they can achieve this status only by gambling that a quasi-marital union may develop into a marriage.⁵³ Finally, most people do marry eventually, and the legal, monogamic union is clearly the ideal.⁵⁴

DIFFERENTIAL INTENSITY OF NORM COMMITMENT

Several conclusions and problems emerge from such a confrontation of general assertions with specific observations. In order to proceed to further related propositions, these conclusions may be summarized: (1) Unequivocally, Malinowski's Principle of Legitimacy holds even for these societies, for which various observers have asserted that it did not hold. Birth out of wedlock is not a "cultural alternative." There is no special approval of the consensual union, no "counter-norm" in favor of such a union. Of

⁴⁹ Raymond T. Smith, *The Negro Family in British Guiana*, New York: Grove Press, 1956, pp. 109, 149, 182.

⁵⁰ *Ibid.*, pp. 126, 145, 132.

⁵¹ *Ibid.*, pp. 102, 120, 156, 178.

⁵² *Ibid.*, pp. 179-180; see also pp. 59, 148-149.

⁵³ *Ibid.*, p. 138. The highest illegitimacy rate occurs among births to females 15-19 years of age; British Guiana, *Annual Report . . .*, *op. cit.*, p. 9.

⁵⁴ *Ibid.*, Chapter 5.

course, the parental anger aroused by a clandestine pregnancy will not be repeated when the girl has entered a consensual union. Nevertheless, in none of these societies does the unmarried mother or her child enjoy the same status as the married mother and her legitimate children. A union based on a marriage enjoys more respect than do other types of unions. (2) Equally clear, however, is the corroboration of another principle: that the degree of norm commitment varies from one segment of the population to another. Not only do some individuals reject particular norms, but the members of some strata are less concerned than those of others about given norms.⁵⁵ (3) A more specific inference from the latter principle is also corroborated, namely, that the lower social strata are less committed than the middle or upper strata to a variety of family norms, in this instance that of legitimacy,⁵⁶ and also obey them less.

More important, however, is a reformulation of Malinowski's principle. As stated, it gives too little emphasis to the real foundation on which it rests, and ignores the differences in norm commitment among different strata, doubtless because neither problem was important in the societies with which Malinowski was concerned. The principle in fact rests primarily upon the function of status placement, not that of locating a father as "protector": the bastard daughter of a count is still illegitimate even if he "protects" her. Violation of the norm creates some status ambiguity with respect to the child, the parents, and the two kin lines. Consequently, (4) commitment to the norm of legitimacy will be greater among the

strata or kin lines which enjoy a higher prestige, or in which concern with the kin relation is higher. Although in general this concern is more marked in the upper strata, in every stratum there will be some family lines which possess "traditions," pride, a sense of kin identity, and so on. Illegitimacy rates can be expected to be higher among the lower strata in all societies. (5) Correlatively, to the extent that a given society possesses a high proportion of lower strata families who are concerned little or not at all with their lineage, that society will exhibit a higher total rate of illegitimacy than it would have if the proportion were lower.

Given a high rate of illegitimacy, two further inferences may be made. (6) The actual amount of stigma suffered by the average illegitimate child cannot be great, relative to legitimate children in his same stratum and neighborhood. (7) The "matrifocality" of the Caribbean family is merely the result of the mother being left with her children, by either a casual lover, a consensual partner, or husband. The "matriarch" who is in charge has power precisely because no other adult of her generation is there to exercise it. Very likely a different personality configuration as well as a different self-image can and sometimes does develop from this experience.⁵⁷ The loyalty of children to the mother is stronger under such a system, since the father is not likely to be around during much of the infancy and youth of the offspring.⁵⁸

On the other hand, early in the union, or continuously when the father remains in the union, the male behaves in a fashion which might be called "patriarchal" in the United

⁵⁵ Thus, one can find individuals who specifically reject marriage for one reason or another in all these societies. However, the empirical question is: what percentage of the society or stratum? In our society, too, any public opinion poll will locate a few such individuals.

⁵⁶ There is substantial literature on this point. See, e.g., William J. Goode, *After Divorce*, Glencoe, Ill.: Free Press, 1956, Chapters 4 and 5; Ruth S. Cavan, *The Family*, New York: Crowell, 1953, Chapters 5, 6, and 7; and William F. Whyte, "A Slum Sex Code," *American Journal of Sociology*, 49 (July, 1943), pp. 24-31. For other data relevant to the subsequent discussion, see Herbert Hyman, "The Value Systems of Different Classes . . ." in R. Bendix and S. M. Lipset, editors, *Class, Status, and Power*, Glencoe, Ill.: Free Press, 1953, pp. 426-442.

⁵⁷ Nor should the matter of *self-selection* should not be forgotten. Given the social option, some individuals will find this role more congenial and choose it against other alternatives.

⁵⁸ Although almost every writer points to "some" consensual unions which have "lasted as long as" legal ones, the instability of both types seems indubitable, and consensual unions are less stable; see R. T. Smith, "Family Organization in British Guiana," *Social and Economic Studies*, 1 (No. 1, 1953), p. 101; Simpson, *op. cit.*, p. 656; Braithwaite, *op. cit.*, p. 147; Seda, *op. cit.*, p. 293; Mintz, *op. cit.*, p. 375; Stykos, *op. cit.*, p. 119; Simey, *op. cit.*, p. 16. (Kreiselman, by contrast, asserts stability for both types: *op. cit.*, p. 180). That matrifocality is by default has been noted by others, e.g., Kreiselman, *op. cit.*, p. 282; Simey, *op. cit.*, p. 43; Braithwaite, *op. cit.*, p. 147.

States. It is possible that some observers have been misled, in their evaluation of the mother's power, by a false image of male behavior in such patriarchal societies as Japan, China, and India, where in fact the older mother is likely to have great authority in the home even when she pays considerable overt deference to the male head of family.

ROLE BARGAINING AND ILLEGITIMACY

An "explanation" of these high rates may properly take two directions. One of these would widen our empirical perspective to include other areas of the world, especially the countries south of the Rio Grande where high illegitimacy rates are found, and locate the cultural elements which are common to them. In a related paper, I am making such an analysis, with special reference to the cultural structure of a society and conformity to its norms. This analysis seeks to answer the question: in what types of societies are high rates found?

The second direction is to focus on the more immediate social forces which create a high illegitimacy rate in the Caribbean. It may be granted that the lower norm commitment in the lower strata of this area would, other things being equal, decrease conformity. Intensity of norm commitment, however, is only one element in the decision to risk pregnancy. The social pattern of primary importance is that the young woman in her courtship behavior must make essentially an *individual role bargain*. This apparent contrast with courtship patterns which produce low illegitimacy rates requires only little attention.

By "making an individual role bargain," I refer to the fact that in any role relationship both ego and alter are restricted in what services they may agree to perform for one another, by the expectations of others and thus by the sanctions which others will apply. For example, father and daughter owe, and feel they owe, certain obligations to one another, and in part these obligations are met because of the rewards and sanctions which either can direct toward the other. However, even if both of them are willing to agree to a different set of obligations—say, those appropriate to lovers—there is a "third layer" of persons who have role relationships with either ego and alter,

or both of them, and who will act to force both of them to perform properly. These actions include pressures on ego or alter to punish the other for improper performance.

All courtship systems are market systems, in which role bargains are struck. They differ from one another with respect to the commodities which are more or less valuable on that market (beauty, personality, kinship position, family prestige, wealth) and who has the authority to do the marketing. Modern Western societies seem to constitute the only major historical civilization in which youngsters have been given a substantial voice in this bargaining (Imperial Rome might be added by some historians). Even in the U. S., however, where this trend is most fully developed, numerous studies have shown that youngsters make their choices within a highly restricted market, with respect to age, race, religion, social class, and so on. Precisely because courtship systems are bargaining systems, apparently hypergamous marriages (the woman marries upward in class) usually are, in most societies, unions in which a high ranking on one or more variables (wealth, beauty) is traded for a high ranking on other variables (power, prestige, race).⁵⁹ As a consequence, most marriages occur between individuals of like rank, or at least like bargaining power,⁶⁰ whether youngsters or their elders have the greater authority to conduct the bargaining process. When one party has much less bargaining power, he may be unable to pay as much as the other demands, or will have to pay much more than another family with greater bargaining power.

Although these principles hold with respect to both the choice of marital partner and the decision to marry at all, they are upheld, as is any market system, by a set of community-wide or stratum-wide set of agreements about *what* is valuable and *how* valuable those characteristics are, and a set of corresponding pressures which prevent the individual from paying too much. In our society, for example, even if a middle-class

⁵⁹ See Davis, *op cit.*, p. 386.

⁶⁰ Of course, the principle of least interest operates in courtship as in marital conflict; the individual who is more deeply in love has less bargaining power. Willard Waller and Reuben Hill, *The Family: A Dynamic Interpretation*, New York: Dryden, 1953, pp. 190-192.

girl is willing to bear a child outside of marriage, usually her parents will oppose this behavior strongly because she would be giving more than need be under the operating market system.

By contrast, what is striking in the Caribbean community studies are the anonymity and isolation within which the decision is made to enter a union, and the fact that under those social conditions the girl has little chance of being married at all unless she is willing to risk a union outside of marriage. Not only does she become pregnant without her parents' knowing that she is courting, but she is also likely to enter the consensual union without any prior ritual or public announcement.⁶¹

A synthesis of the factors of importance in the decision to marry or to enter a consensual union can be made from the existing studies (although in many cases needed data are lacking because the appropriate questions were not asked⁶²). Especially important are the following five points:

1. The class pattern of marriage has been suggested above. This may be clarified here by noting that not only do middle- and upper-class individuals marry (though of course males from those strata may have mistresses whom they do not marry), but that most members of the lower strata also marry eventually. Some lower class persons never enter a consensual union, but begin their conjugal career by a wedding. Others begin with a consensual union, but marry sooner or later, usually after the male has somewhat improved his social position. In certain communities which seem to enjoy a higher social standing, a substantial majority of all marital unions are legal.⁶³

2. Kreiselman's finding for Martinique concerning marriages between persons of similar rank can be extended to every Caribbean community. Notwithstanding the frequently voiced assumption to the contrary, many fine distinctions of prestige are made within the lower class, in spite of its apparent homogeneity to the (usually White)

outside observer.⁶⁴ If there were no other index, we could rely on the fact that certain members of the lower class do marry without entering a consensual union.⁶⁵ However, other data are also available, for example, the higher ranking of unskilled laborers with *steady* jobs. Granted, these differences are less sharp or refined than the gross differences between upper and lower strata, but within the narrower class horizon of persons in the bottom stratum they may nevertheless loom large. From this fact, we can suppose that when marriage does occur, the man and woman are more likely to be "rank equals," within the more generalized terms proposed above—which include not merely family prestige but also personal qualities such as beauty.⁶⁶

3. Given a system in which consensual unions are common, it follows that the punishments for entering them cannot be severe, and the rewards for marrying cannot be great. (This proposition is an inference from a well-known principle of social control.) Consequently, the girl's parents or relatives (there is no extended kin group which acts as a unit) are punished or rewarded very little if, in turn, they make or fail to make her behavior conform to "ideal" norms.

4. In the Caribbean, there is no "free" adolescent courtship system such as our own, in which an as yet ineligible male is permitted to approach an immature girl, under the protection of her relatives and peer group. Many or most of the men she first meets are ineligible because of the great cost of a wedding. Most of them have not accumulated enough wealth to finance the formal union and to support its subsequent requirement of a higher level of living than a consensual union.⁶⁷ Consequently, the girl's

⁶⁴ For example, although Smith, *op. cit.*, pp. 218-220 *et passim*, refers to a lack of status differentiation, his detailed descriptions show considerable differentiation.

⁶⁵ See, for example, Smith's description (*ibid.*, pp. 169-170) of a formal engagement; Seda's comment that parents may insist on a wedding ceremony (*op. cit.*, p. 293); and Clarke, *op. cit.*, pp. 85-88.

⁶⁶ Here the variable of rank is generalized, of course, and Kreiselman's observation (*op. cit.*, p. 278) from Martinique is extrapolated to the rest of the Caribbean.

⁶⁷ Cf. Clarke, *op. cit.*, pp. 78, 99; Herskovits and Herskovits, *op. cit.*, p. 84.

⁶¹ Smith, *op. cit.*, pp. 101, 137.

⁶² For Jamaica, as noted in footnote 28, the most complete synthesis has been made by Blake, *op. cit.*

⁶³ E.g., Orange Grove reported in Clarke, *op. cit.*; Better Hope reported in Smith, *op. cit.*; and apparently San José as reported in Wolf, *op. cit.*

first love and sex contacts occur away from home, and without the knowledge of the family. These first contacts take place essentially in social anonymity, so that she must make the best bargain she can, without the family's support.⁶⁸ Parental anger, reported in most studies, is at least in part a reaction to the knowledge that the girl has entered the world of adulthood without parental permission and has acted independently while presumably still a child.⁶⁹

5. The Caribbean girl with unusual qualities may be able to demand marriage. However, the average girl has little chance at marriage, early or late, unless she is willing to gamble that a more permanent union will grow from one relationship or another. Without reliable data on the number of unions in the average individual's life, we cannot state what these chances are. Motherhood lowers the girl's value in the market, but if she does not produce a child for the man with whom she is living, or with whom she has a liaison, her chance of a stable union is low.⁷⁰ The decision to marry, within the existing social structure, is his rather than hers, and she gains more from marriage than he does. Consequently, as noted previously, it is the women who press toward marriage, while they must take the only road which can—and, apparently, eventually does—lead to marriage. Meanwhile, however, a woman may have children by several men, and may leave some or all of them with her parents or relatives when

entering a new union⁷¹—a practice often resulting in the "grandmother" family. The widespread adoption pattern in the Caribbean is in part a method of taking care of these children. Ideally, a man wants only his own children in his home, especially if he is marrying.

SUMMARY

Although Malinowski's Principle of Legitimacy has been called into question by several students of the Caribbean, the detailed descriptions of family and courtship patterns in that area show that it is generally valid. Derived from societies in which conformity to this norm was high, however, the principle requires revision. This should emphasize status placement rather than "paternal protection," and should specify the lower strata as the part of the society in which deviation from the norm is greatest. In addition, revision of the principle should note the weaker norm commitment in these strata, and the resulting lowering of both punishment for deviation and reward for conformity.

The "matrifocal" Caribbean family is a product of an unstable family pattern, in which the mother or grandmother is often in power because no father is there. The courtship pattern is anonymous, so that the young girl must make the best bargain she can, which usually means that she must be willing to risk pregnancy in order to establish a basis for a more stable union. Eventually, most individuals do enter a marriage. The girl is not protected by her relatives or peers in this bargaining. Thus, though the Principle of Legitimacy is valid, it must be revised. It has also been shown how courtship relations in the Caribbean may lead to a high illegitimacy rate, even when the norm of legitimacy is accepted.

⁶⁸ Kreiselman, *op. cit.*, p. 99; Herskovits and Herskovits, *op. cit.*, p. 88; Smith, *op. cit.*, pp. 109, 137, 145.

⁶⁹ Smith, *op. cit.*, p. 145, makes this point clearly, citing a common statement, "If you want to play a big woman go find yourself a man."

⁷⁰ At the same time a pregnancy may frighten him away, as being too great a burden to assume. Clarke, *op. cit.*, pp. 75, 91, 100-102; Smith, *op. cit.*, p. 138. Blake, *op. cit.*, also reports this fact.

⁷¹ Herskovits and Herskovits, *op. cit.*, pp. 104-105, 131; Clarke, *op. cit.*, p. 91; Smith, *op. cit.*, Chapter 4.

CULTURAL RELATIVISM AND PREMARITAL SEX NORMS *

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By means of "record linkage," it was possible to measure and compare the phenomenon of premarital pregnancy across three widely divergent Western cultures: Utah, with its very conservative sex norms; Indiana, which is approximately average for the United States; and Denmark, which typifies the rather liberal sex codes of Scandinavia. Not only did Denmark show the highest incidence of premarital pregnancy, and Utah the lowest, but the premaritally pregnant couples in Denmark tended to take their time about getting married, and tended not to let the pregnancy affect them negatively to the point of divorce (in comparison with those from the other two cultures). Thus, sexual permissiveness was found to be associated with higher incidences but lower negative effects of premarital pregnancy. Nevertheless, not every aspect of the phenomenon was culturally relevant.

IN noting that behavioral standards vary over time and from society to society, William Graham Sumner made the now classic statement: "The mores can make anything right." By this he meant that moral problems are interpreted differently by different societies—that questions of right and wrong are relative to the particular culture in which the behavior occurs. This theory has been labeled *cultural relativism*. It challenges the notion of absolute standards of judgment to be applied uniformly regardless of time or place.

But there has been little quantitative research to test the theory of cultural relativism, especially as applied to modern Western societies. Furthermore, the tendency has been to stop with a simple noting of attitudinal and behavioral differences, without pinning down the relativism of the consequences of these differences. For example, it is well known that some societies are rather

restrictive and others very permissive regarding premarital sexual behavior;¹ but there is almost no information as to whether this behavior has the same or different effects (in terms of mental health, subsequent social behavior, or both) across these contrasting types of societies.

This paper is an attempt to illuminate further the notion of cultural relativism by applying it to differing sets of premarital sex norms. Since premarital pregnancy can be reliably measured by use of a method known as "record linkage,"² whereas most other levels of sexual behavior are more elusive, the focus here is upon this phenomenon.³ We are interested in both regularities and variations among the cultures studied, with special reference to the consequences of premarital pregnancy.

Specifically, it is hypothesized that the more permissive the culture regarding sexual matters, the greater will be the incidence of premarital pregnancy, *but the lesser will be the effects of such pregnancy as pressure either for hasty marriage or for subsequent divorce*. It is further hypothesized that certain aspects of premarital pregnancy are not culturally relevant.

* Slightly revised version of a paper read at the Fourth World Congress of Sociology, sponsored by the International Sociological Association, September, 1959.

In an earlier paper prepared for the Fourth International Seminar on Family Research, held at Wageningen, Holland, two years ago, the writer presented certain preliminary data on the sex norms of the same cultures treated here and sketched in his "theory of value relevance." Since that time, more data bearing on the problem have been gathered and the analysis has been extended. The present report is a continuation of the previous one, and it is expected that still others will follow. See Harold T. Christensen, "Value Variables in Pregnancy Timing: Some Intercultural Comparisons," in Nels Anderson, editor, *Studies of the Family*, Göttingen, Germany: Vanderhoeck & Ruprecht, 1958, Vol. III, pp. 29-45.

¹ See e.g., George Peter Murdock, *Social Structure*, New York: Mcmillan, 1949, pp. 260-283.

² For descriptions of this method, see Christensen, *loc. cit.*; and Harold T. Christensen, "The Method of Record Linkage Applied to Family Data," *Marriage and Family Living*, 20 (February, 1958), pp. 38-43.

³ It is expected that the questionnaire and interview data that were collected to throw light upon other aspects of sexual behavior in these same cultures, will eventually be reported also.

SEX NORMS IN THREE CULTURES⁴

In order to treat culture as a variable, we have made identical observations in three widely divergent areas. The first is the state of Utah, where the Mormon Church is dominant and premarital sex norms tend to be extremely conservative, almost to the point of being puritanical. Here, religion is a motivating force in the lives of most people, and the religious interpretation of premarital sexual intercourse is that it is an extremely grievous sin. Waiting until marriage for sexual intercourse—"keeping the law of chastity"—is regarded as one of the highest of virtues.⁵

The second is the state of Indiana, which in many ways is typical of the United States as a whole. It is centrally located and heterogeneous in culture. It is approximately an average state in size, in rural-urban distribution, in population numbers and composition, and in various social indices such as median income, school attendance, and marriage, birth, and divorce rates. The "chastity norm" is a part of the prevailing culture in Indiana as in most of the United States—in prescription even if less so in practice. And there are religious incentives, promoted by a variety of denominations, which give support to the sexual mores.

The third location is Denmark, which, like all of Scandinavia, has a long tradition of sexual intercourse during the engagement. This goes back three or four centuries at least, in spite of efforts by the State Lutheran Church to establish a chastity code.⁶ In this connection, it is important to point out that, although most Danes have their names on

the church records, they seldom attend church services. Except for a few, religion in Denmark is not a strong motivating force in the lives of the people. Croog notes the importance of understanding the *ring engagement*—which has almost the status of a formal marriage, including rights to sexual intercourse, and obligations to marry if pregnancy results—as background for interpreting sexual behavior in Denmark. He also explains how this pattern of sexual freedom is spreading to include the more informal "going steady" relationships; and how these practices are encouraged by a liberal clergy, by welfare laws which make abortion and unmarried motherhood relatively easy; and by the facility with which premarital sexual behavior can be rationalized since "everyone is doing it."⁷ Svalastoga cites five recent empirical studies to support his claim that: "Coitus before marriage may now safely be considered the rule and chastity the exception in Scandinavia."⁸

Thus these three areas have widely different norms regarding premarital sexual behavior. At the one extreme is Utah, dominated by a homogeneous culture and conservative religious tradition. There, the moral condemnation of premarital sexual intercourse under all circumstances has the support of strong supernatural sanctions. At the other extreme is Denmark, with a liberal tradition in sexual matters, and a religious membership, which, though homogeneous, is only nominal. There, premarital sexual intercourse, if accompanied by love and the intent to marry, tends to be an accepted practice. Somewhere in between these two extremes lies Indiana, with norms more moderate, more variable, and yet somewhat typical of the country of which it is a part.

INCIDENCES OF PREMARITAL CONCEPTION

For any accurate measure of premarital conception, one needs to know three quantities: abortion among the unmarried, both spontaneous and induced; illegitimacy, that is, birth outside of marriage; and the num-

⁴ For more detailed descriptions of these differing sex norms, see Christensen, "Value Variables . . .," *op. cit.*, pp. 30-35.

⁵ In this connection, it is interesting to recall Kinsey's finding to the effect that religiously devout men and women participate less in all socially disapproved forms of sexual behavior. He regarded religion as being the "most important factor in restricting premarital activity in the United States." See Alfred C. Kinsey et al., *Sexual Behavior in the Human Female*, Philadelphia: Saunders, 1953, pp. 324, 686-687, and *passim*.

⁶ Cf. K. Robert Wikman, *Die Einleitung der Ehe*, Aabo, Finland: Acta Academiae Aaboensis, XI, 1937; and Georg Hansen, *Sædelighedsforhold Blandt Landbefolkningen i Danmark i det 18 Aarhundrede*, Copenhagen: Det Danske Forlag, 1957.

⁷ Sydney H. Croog, "Aspects of the Cultural Background of Premarital Pregnancy in Denmark," *Social Forces*, 30 (December, 1951), pp. 215-219.

⁸ Kaare Svalastoga, "The Family in Scandinavia," *Marriage and Family Living*, 16 (November, 1954), pp. 374-380; quotation from p. 337.

TABLE 1. SELECTED INDICES OF PREMARITAL CONCEPTION

Indices	United States		Denmark	
	Utah County, Utah	Tippecanoe County, Indiana	City of Copenhagen	Entire Country
I. Illegitimacy Rate ¹	.9	2.9	11.2	6.6
II. Premarital Conception Rates ²				
A. Child Born Within First 6 Months of Marriage	9.0	9.7(10.0)*	24.2(31.1)*	32.9(34.9)*
B. Child Born Within First 9 Months of Marriage	30.9	23.9(26.1)*	30.5(39.3)*	44.3(48.5)*

¹ Per cent of all births occurring outside of wedlock. Calculations based upon official reported statistics for the year 1955.

² Per cent of marital first births occurring within six and nine months of the wedding, respectively.

Figures in the first three columns are from the Utah County, Tippecanoe County, and Copenhagen studies described in the paper. Fourth column figures were derived from the *Statistisk Aarbog 1956*, Copenhagen: Bianco Lunos Bogtrykkeri, 1956, Table 25, p. 35, and are for the calendar year 1955.

* Figures in parentheses are adjusted indices, derived by using only those births which occurred during the first four years of marriage as the base for calculation. This adjustment is for the purpose of making the figures comparable in this respect with the Utah indices shown in the first column.

ber of weddings that are preceded by pregnancy.⁹

Unfortunately, there are no available statistics to enable us to make comparisons on the relative numbers of abortions.

With regard to illegitimacy, official statistics for 1955, which are typical of recent years, show the per cent of all births occurring outside of wedlock to be .9 for Utah, 2.9 for Indiana, and 6.6 for Denmark. (See Table 1.) Thus, in these societies, illegitimacy increases with each advance in the sexual permissiveness of the culture.

Although illegitimacy rates can be obtained from published statistics for whole populations, it has been necessary to conduct sample studies for measures of the premarital conceptions which end in postmarital births.¹⁰ As a consequence, the following analysis relies heavily upon the

writer's earlier record linkage studies of Utah County, Utah, and Tippecanoe County, Indiana, and his more recent parallel investigation of Copenhagen, Denmark. The Utah County data were derived by comparing marriages occurring during the years 1905-7, 1913-15, 1921-23, and 1929-31 with birth records for four years following each wedding, in order to find the date of the first birth. This process yielded 1,670 cases. The Tippecanoe County data were derived by taking marriages which occurred during the years 1919-21, 1929-31, and 1939-41, matching them with the birth records searched for five years following the wedding, and finally checking against the divorce records to discover which marriages ended in failure. The result consisted of 1,531 cases involving a first child, with 137 of these cases terminating in divorce. The Copenhagen data were derived by taking every third marriage which occurred during a single year, 1938, eliminating cases involving remarriage and those in which the wife was thirty or more years of age, and then checking both birth and divorce recordings for sixteen years following the wedding. These steps provided a sample of 1,029 cases involving a first child, with 215 ending in divorce.

These samples from three cultures are not, of course strictly comparable. They were drawn in slightly different ways and have somewhat different compositions. Nevertheless, the contrasts reported below

⁹ Strictly speaking, early birth within marriage provides the only available accurate measure of premarital conception. Some unmarried women have abortions and illegitimate births; the term "premarital" hardly describes them. Yet, since it is likely that the majority of such women later get married, no great violence is done in using the concepts in this way. In Denmark, for example, one study has shown that by age six well over half of all children born out of wedlock are then living with their mother who has since been married—in most cases to the child's father. See "Den Familiemaessige Placering af Børn Født Uden For Aegteskab," *Statistisk Maanedsskrift*, 30 (No. 9, 1954), pp. 193-195.

¹⁰ Denmark has published nation-wide statistics on this phenomenon, but the United States has not.

TABLE 2. FACTORS ASSOCIATED WITH PREMARITAL CONCEPTION *

Factors	Per Cent of First Births Premaritally Conceived ¹		
	Utah County, Utah	Tippecanoe County, Indiana	Copenhagen, Denmark
Wife's Age at Marriage			
Young Group ²	14.4	13.2	29.0
Older Group	7.7	5.7	15.1
Type of Ceremony ³			
Civil	16.6	21.0	37.0
Religious	1.1	9.9	13.5
Husband's Occupation			
Laborer	17.9	16.0	30.0
All Other	8.6	7.2	18.2

* All factor differences were found to be statistically significant in all three samples.

¹ As used here: per cent of marital first births within the first 196 days of marriage (Utah and Indiana), or within the first 182 days of marriage (Denmark).

² This group was defined in the Utah sample as aged 20 or under, in the Indiana sample as aged 24 or under, and in the Danish sample as aged 23 or under.

³ In the Utah sample, the division is not strictly civil-religious, but rather nontemple-temple. Since the most orthodox Mormons marry in one of the temples, this group would represent the most religiously motivated of the religious marriages. But the non-temple group, though mostly civil marriages, would include some religious marriages—where these did not take place in a Mormon temple.

are of sufficient magnitude to suggest at least tentative answers to the problem posed.

From Table 1, it may be observed that the same general pattern holds for this phenomenon as was previously noted for illegitimacy. The six months index, which is a sure minimum measure of premarital conception, makes the clearest comparison. It shows the lowest incidence of premarital pregnancy in Utah, a somewhat higher incidence in Indiana, but a considerably higher incidence in Denmark. The nine months index is less valuable since it includes unknown numbers of postmarital conceptions.¹¹ The higher rate for Utah than for Indiana may simply reflect the tendency to earlier postmarital conceptions in Utah.¹²

These findings may be viewed as validation for our earlier labelings. In these cross-cultural comparisons, behavior has been

found to be consistent with attitudes; and, attitudes *plus* behavior have differentiated Utah and Denmark at opposite ends of a continuum describing premarital sex norms, with Indiana in between.

An interesting contrast between Copenhagen and the whole of Denmark can be seen by comparing the last two columns of Table 1. Copenhagen shows higher illegitimacy rates than the national figures, but relatively low rates of premarital conception. Though interpretation of the contrast takes us beyond available data, we hazard a guess: Copenhagen, being metropolitan in character, reflects the more liberal sex culture, including a sophistication which discourages rushing into marriage just because of pregnancy; thus, it is possible that disproportionately more premaritally pregnant couples in Copenhagen either put off marriage until after the child is born or elect against it entirely. An alternative possibility is that women with an illegitimate pregnancy tend to move to Copenhagen sometime before the child is born.

ASSOCIATED FACTORS

Not only does the incidence of premarital pregnancy differ from culture to culture, as demonstrated above, but it varies among certain sub-groups within each culture. As shown in Table 2, there are strong and con-

¹¹ The normal period of uterogestation in human beings is 266 days, or slightly less than nine calendar months. Furthermore, premature births would cause a number of early postmarital conception cases to be included in this index.

¹² Cf. Christensen, "Value Variables . . .," *op. cit.*, pp. 35-38. See also Figure 1 of the present paper, where Utah is shown to have proportionately more conceptions at the time of marriage than Indiana. Of course, some of these may actually be premarital by a day or so, but the other explanation for the Utah-Indiana differential seems more plausible to the writer.

sistent tendencies for premarital conception to be higher with young age at marriage in contrast to the older ages, with a civil wedding in contrast to the religious ceremony, and with a laboring occupation in contrast to the more skilled and professional ways of earning a living. Each of these differences was found to be in the same direction and to be statistically significant for each of the three cultures studied, which is evidence of certain cross-cultural regularities.

Perhaps youth gets into difficulties of this kind because of its lack of sophistication. Furthermore, since premarital pregnancy encourages earlier marriages than couples otherwise would undertake, marriages of this sort are certain to involve more of the younger-aged persons. The higher proportions of premarital pregnancy among those who have a civil wedding may possibly be explained by a relative lack of religious influence in the lives of these people, plus an attempt on the part of those who become pregnant to hurry the wedding and to avoid the judgment of the church or the scorn of fellow church members. The disproportionately high premarital pregnancy percentages for persons in the laboring occupations may largely be due to the greater sexual permissiveness found in the lower social classes, plus their relative lack of education. But, whatever the complete explanation, there is the strong suggestion here, that broad cultural norms may be to some extent overruled by the operation of other factors.¹³

EFFECTS UPON TIMING OF THE WEDDING

The tendency to be philosophical about a premarital pregnancy when it happens, so as not to be stamped into a marriage, seems to be much more characteristic of Denmark than of Indiana or Utah. It is suggested, perhaps, by the higher Danish illegitimacy rates.

But even stronger evidence is presented in Figure 1, which has been constructed from estimated dates of conception calculated by counting back 266 days from each

date of birth. It may be noted that, whereas in the Utah and Indiana samples the modal time of conception is one lunar month after marriage, in the Danish sample it is five lunar months *before* the marriage. As a matter of fact the Danish data show many more couples conceiving about five months before the marriage than at any other time; in that culture, therefore, premarital conception coupled with subsequent delayed marriage must be considered as the norm. The Indiana curve is bimodal, with the peak for premarital conceptions at two lunar months prior to marriage—suggesting a tendency to get married as soon as possible after the second menstrual period has been missed and the doctor's positive diagnosis has been given. The Utah curve starts low and moves up regularly until the time of marriage and immediately thereafter, when it is the highest of all three.

The fact just noted is further evidence of Utah's pattern of early conception following the wedding. Of the three cultures here compared, Utah has not only the lowest rates of premarital conception but the highest rate of *early* postmarital conception.

Apparently, in Denmark there is little pressure to hurry marriage merely because of pregnancy.¹⁴ In Indiana the tendency is to marry immediately after the pregnancy is definitely known so as to hide the fact from the public. Couples who have premarital sexual intercourse in Utah, on the other hand, seem to hurry marriage because of that fact alone, without waiting for pregnancy to force them into it (religious guilt is a sufficient sanction once the "law of chastity" has been broken).¹⁵

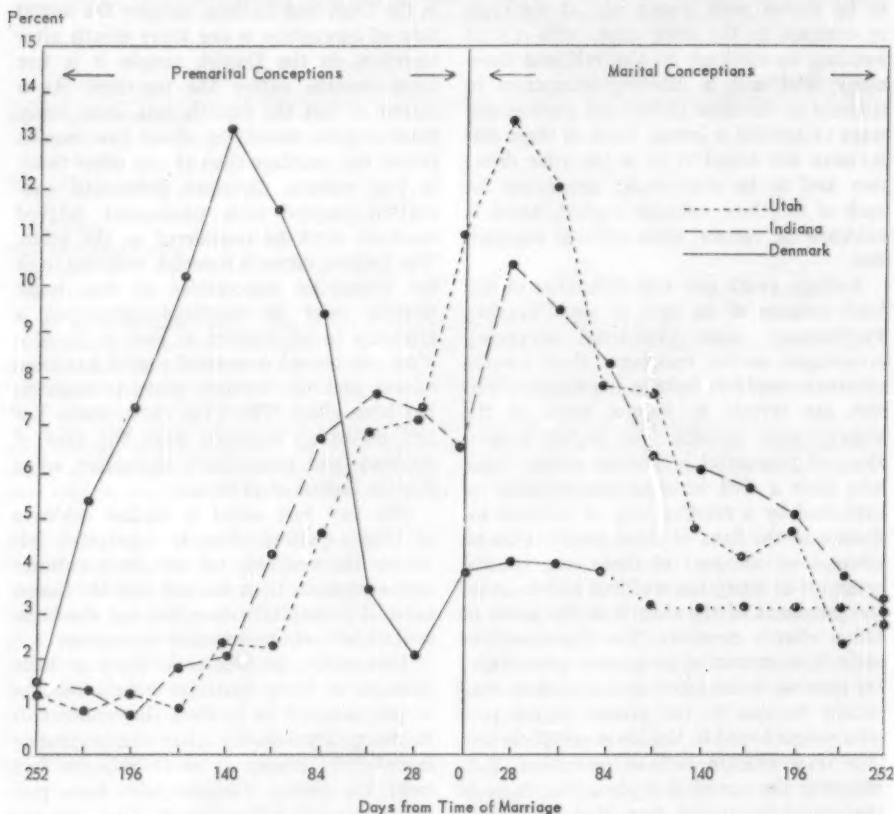
As Kinsey has pointed out, "The psychological significance of any type of sexual activity very largely depends upon what the

¹⁴ In attempting to explain this situation to the writer, several Danish scholars have pointed to the current great housing shortage in Copenhagen—which means waiting for a place to live, thereby discouraging any rush into marriage. When reminded that the figures used here are for 1938 marriages, however, these observers were quick to admit that the argument doesn't apply, since there was little housing shortage then.

¹⁵ Although this latter explanation is speculative, it is plausible. Chastity is so stressed in Mormon culture that the religiously oriented offender may panic and try to ease his conscience by getting married.

¹³ Of course, these factors also have cultural content, but they—and possibly many others—seem not to be confined by the limits of an area or a society; hence the suggestion of cross-cultural regularities.

FIGURE 1. PREGNANCY INCEPTION AS RELATED TO TIME OF MARRIAGE: A CROSS-CULTURAL COMPARISON



(Data are for births occurring during the first nineteen lunar months of marriage and are expressed as percentages.)

individual and his social group choose to make of it."¹⁶ Since in Danish culture there is less stigma placed on premarital conception and on illegitimacy than in Indiana, and especially in Utah, the differences in timing pattern for the wedding once pregnancy has occurred may be explained in cultural terms.

EFFECTS UPON THE DIVORCE RATE

This type of explanation may also apply to possible variations in divorce rate differentials of premarital pregnancy *versus* postmarital pregnancy cases. We would hypothesize that the more liberal the culture

the *less* likely is premarital pregnancy to be followed by divorce. This hypothesis is tested with data from the Indiana and Danish samples.¹⁷

For Tippecanoe County, it has been reported earlier that the divorce rate is significantly higher for premarital than postmarital pregnancy couples.¹⁸ For marriages occurring in Copenhagen during 1948, Holm

¹⁷ Unfortunately, in our Utah sample record linkage was limited to marriage and birth data; whereas in the Indiana and Denmark samples divorce data were also included.

¹⁸ Harold T. Christensen and Hanna H. Meissner, "Studies in Child Spacing: III—Premarital Pregnancy as a Factor in Divorce," *American Sociological Review*, 18 (December, 1953), pp. 641-644.

¹⁶ Kinsey, *op. cit.*, p. 320.

TABLE 3. DIVORCE RATE COMPARISONS BY INTERVAL TO FIRST BIRTH
(For Births Occurring Within 5 Years of the Wedding)

Classification	Copenhagen, Denmark			Tippecanoe County, Indiana		
	Number of Cases	Number Divorced	Per Cent Divorced	Number of Cases	Number Divorced	Per Cent Divorced
Interval Between Marriage and First Birth						
(1) 0-139 days (premarital pregnancy, marriage delayed)	176	60	34.1	71	14	19.7
(2) 140-265 days (premarital pregnancy, marriage hurried)	129	31	24.0	276	39	14.1
(3) 266 days-4.99 years (postmarital pregnancy)	572	111	19.4	1184	84	7.1
Percentage Difference Between Divorce Rates						
(4) Between lines 2 and 1			42.1			39.7
(5) Between lines 3 and 2			23.7*			98.6*

* No direct formula has been located for testing the statistical significance of this intersample difference between differences in proportions. However, an approximate equivalent test is to consider $p_1 - p_2 < p_3 - p_4$ (where $p_1 = 24.0$, $p_2 = 19.4$, $p_3 = 14.1$, and $p_4 = 7.1$). When the p-values are changed according to the arcsine transform, they have been placed on a comparable scale and the use of the normal probability table is permitted. This procedure yields a probability for a one-tailed test of .12.

Alternatively, we may approximately test $\frac{p_1}{p_2} < \frac{p_3}{p_4}$ if we assume $\frac{p_1}{p_2} - \frac{p_3}{p_4}$ is normally distributed, and use an approximate variance formula. Since the hypothesis is stated in terms of the greater ratio for the United States than for Denmark, a one-tailed test is permissible, yielding a probability of .038. There is no way to evaluate the assumption of a normal distribution for this test.

has shown that, with age controlled, the divorce rate is not significantly different for couples bearing a child within the first nine months of marriage than for all other cases.¹⁹ At first glance, this seems to bear out our hypothesis.

It is to be noted, however, that Holm did not compare premarital pregnancy cases with postmarital pregnancy cases, as was done for Tippecanoe County, but rather with all non-premarital pregnancy cases, including childless couples. Since those who become divorced are less likely to have children than those who do not,²⁰ the inclusion of childless cases in the non-premarital pregnancy category would raise the divorce rate for that category and, in this way, would obscure

the true comparison. What is needed is a comparison between divorce rates of premarital and postmarital pregnancy cases; for unless non-conceivers are excluded, it is impossible to determine the effects of conception timing.

Table 3 is designed to compare the Copenhagen and Tippecanoe County samples concerning possible effects of premarital and postmarital pregnancy upon the divorce rate. As noted above, these two samples are not strictly comparable, but they are approximately so.²¹ It seems probable that the following generalizations are at least tentatively justified:

(1) In both populations there is the clear tendency for the divorce rate to fall as the

¹⁹ Henry F. Holm, Actuary for the City of Copenhagen's Statistical Office, *Statistisk Maanedsskrift*, 33 (No. 4, 1937), Table 12, p. 117.

²⁰ In the writer's Copenhagen sample of 1938 marriages, for example, 57.0 per cent of the childless marriages ended in divorce or separation as compared with 20.9 per cent of the fertile marriages. A primary explanation for this differential is that many of the divorces occur relatively soon after the wedding, before the couple has decided to start a family.

²¹ See descriptions of the two samples, above. Calculations for Table 3 are based uniformly on cases having a first child born with five years of the wedding. Although absolute divorce rates cannot be compared across the two samples—since they would be influenced in distinctive ways by differential emigration and differential lengths of time of exposure to the divorce possibility—there seems to be no good reason why the relative rates by pregnancy timing cannot be compared.

length of interval between marriage and first birth increases. This means that premarital pregnancy cases are more likely to end in divorce than are postmarital pregnancy cases,²² and that those premarital pregnancy couples who delay marriage for a considerable time after the knowledge of pregnancy have the highest divorce rate of all—in Denmark as well as Indiana.

(2) The *relative* difference in divorce rate between premarital pregnancy couples who hurried marriage and those who delayed it is essentially the same for both populations. Thus, Copenhagen figures show a 42.1 per cent difference between these two rates as compared with a difference of 39.7 per cent in Tippecanoe County, an intersample difference that is not significant.

The facts that both samples show substantially higher divorce rates for couples who delay marriage after knowledge of pregnancy and that the differentials in this respect are about the same in the two cultures suggest universal tendencies for certain pregnant couples to marry under the pressure of social responsibility (for example, sympathy for the lover, consideration for the future child, or parental influence). The data also suggest that, statistically speaking, such "shot gun" marriages do not turn out well.

(3) The *relative* difference in divorce rate between postmarital pregnancy couples and the premarital couples who married soon after the discovery of pregnancy is four times greater in the Indiana sample (98.6 per cent compared with 23.7 per cent), an

intersample difference that by some tests is statistically significant. (See footnote to Table 3.)

The fact that the postmarital pregnancy divorce rate is lower in both cultures is evidence that premarital pregnancy—even when associated with an early wedding—tends generally to make marriage's survival chances less than even. This may be because some marriages take place under pressure from others and are therefore accompanied by resentment, or because in their haste to escape public scorn the couple marries without adequate preparation, or in the absence of love, or in the face of ill-matched personalities. But the fact that the postmarital-premarital pregnancy divorce rate differential is substantially less in Denmark, gives strong support to our hypothesis. It seems probable that in Denmark, where sexual relations outside of marriage are more or less accepted, premarital pregnancy will have less negative effect upon marriage than in Indiana, where it is expected that sexual intercourse and pregnancy be confined to marriage.

SUMMARY AND THEORY

Premarital sex norms in Utah, Indiana, and Denmark stand in sharp contrast—with Utah being very conservative or restrictive, and Denmark being extremely liberal or permissive. As might be expected, therefore, premarital pregnancy rates were found to be lowest in the Utah sample and highest in the Danish sample, with the difference being considerable. Furthermore, certain consequences of premarital pregnancy were found to vary from culture to culture. Thus permissive Denmark, at the time of the study, showed the longest delay between premarital conception and the wedding, and the smallest divorce rate differential between premarital pregnancy and postmarital pregnancy cases.²³ In all three cultures the same factors

²² There is an interesting parallel finding from the Copenhagen data: marriages in which the wife had borne an illegitimate child previously showed a divorce rate of 45.7, as compared with 20.9 for childbearing marriages where she had not. A partial explanation, of course, may be that a selective factor is operating, which may mean that the least stable personalities are the ones most likely to become pregnant before marriage and also to be divorced later. But another possibility is that, through such things as resentment about the necessity to marry, guilt feelings, and poor preparation and unsuitable personality matching because of a hasty or pressured marriage, the premarital pregnancy may itself help to bring about divorce. In the Tippecanoe County study, the writer controlled other divorce-producing factors, through matching, and still found premarital pregnancy to be significantly associated with high divorce; see Christensen and Meissner, *loc. cit.*

²³ As noted above, divorce rate comparison does not include the Utah sample since data were not available. It is believed, however, that the Utah divorce rate differential (between premarital and postmarital pregnancy cases) probably is the greatest of the three areas—because premarital sexual intimacy is most strongly condemned there.

This unestablished assumption can be argued by an analogy. The drinking of alcoholic beverages is

were associated with premarital pregnancy: namely, young age at marriage, a civil wedding, and a laboring occupation.

In some respects our data give support to the idea of cultural relativism. It has been shown that both the rates and effects of premarital pregnancy are to a considerable extent relative to the cultures involved. The most liberal culture was found to have the most premarital pregnancy, but also the least negative effects therefrom; in Denmark there is less pressure than in the American cases either to speed up the wedding or to resort to divorce when premarital pregnancy occurs. Thus, the relationship is not simply a matter of how premarital pregnancy affects subsequent behavior, considered in a vacuum, but rather how it affects this behavior in the light of particular norms. Cul-

tural norms represent an intervening variable.

But there are also *regularities* among the cultures studied. In all of them, pregnancy usually takes place within marriage. In all of them also, premarital pregnancy is found to be associated with young age, a civil wedding, and a laboring occupation. Finally, the Indiana-Denmark comparisons reveal a parallel phenomenon of higher divorce rates for premarital pregnancy than for postmarital pregnancy cases. These rates are especially high, and in similar magnitude within both cultures, for couples who delay marriage until just before the child is born. Forced marriage, in other words, seems to work against marital success regardless of the culture. All of this suggests the existence of certain universals which are to some extent independent of the cultural variable.

The present analysis is concerned with *intercultural* comparisons. The next step is to see if the theory applies to the *inracultural* level, that is, when interpersonal differences are taken into account. We hypothesize both regularity and variability at that level also, with personal values having very much the same effects as cultural norms are found to have in this report.

also strongly condemned in Utah (and in the rest of Mormon culture). Research shows that Mormon college students have the lowest incidence of drinking among religious groups, but that, of the drinkers, Mormon students have a very high rate of alcoholism. This suggests that cultural restrictions can lower the incidence of the condemned practice, but that for those who indulge, the negative effects are apt to be extreme. Cf. Robert Strauss and Selden D. Bacon, *Drinking in College*, New Haven: Yale University Press, 1953, *passim*.

UNMARRIED FATHERS AND THE MORES: "SEXUAL EXPLOITER" AS AN EX POST FACTO LABEL *

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This paper attempts to clarify the normative content of the label "sexual exploiter" as applied to the unmarried father. Age and educational differences between 201 white unmarried father-unwed mother pairs are examined and interpreted as approximating age and educational differences between white husband-wife pairs in the general population. The term sexual exploiter is interpreted as an ex post facto label which serves to support traditional values in mate-selection and marriage, but which also serves to impede research in illegitimacy when accepted at face value.

UNMARRIED fathers are, so to say, half the biological cause of illegitimacy, yet the ratio of studies of them to studies of unwed mothers is approximately one to 25.¹ This disproportionate ratio provided the initial stimulus for the present examination of the normative and research significance of the label "sexual exploiter" as affixed to the unmarried father.

* Part of a larger study made possible by grants from the Rosenberg Foundation of San Francisco and the University of California Institute of Social Sciences. I am indebted to the following persons whose generous cooperation made the study possible: nearly 500 anonymous physicians in Alameda County, California, who obtained and reported data on private cases; Colonel Ruth Pagen and Major Helen Smeitin and her Staff at the Booth Memorial Salvation Army Maternity Home in Oakland, California; and Regina Mendel and her Staff in Medical Social Service at Highland Hospital, Alameda County.

¹ The following number of references, plus perhaps this many more of which the author is unaware, stands in sharp contrast to the several hundred references on unwed mothers: Sarah B. Edlin, *The Unmarried Mother in Our Society*, New York: Farrar, Straus and Young, 1954, Chapter 15, "How Does the Unwed Father Feel?"; Samuel Futterman and Jean B. Livermore, "Putative Father," *Journal of Social Casework*, 28 (May, 1947), pp. 174-178; Ruth Reed, *The Illegitimate Family in New York City*, New York: Columbia University Press, 1934, Chapter 14, "The Unmarried Fathers;" Norman Reider, "The Unmarried Father," *American Journal of Orthopsychiatry*, 18 (April, 1948), pp. 230-237; *The Care of Children of Unmarried Parents*, Chicago: Jewish Children's Bureau of Chicago, 1944, Chapter 3, "The Unmarried Fathers;" Maud Morlock, *The Fathers of Children Born Out of Wedlock*, Washington, D. C.: U. S. Children's Bureau, 1939; Leontine Young, *Out of Wedlock*, New York: McGraw-Hill, 1954, Chapter 8, "The Unmarried Father."

A very brief consideration of several folkways, social practices, and values which serve to define the unwed mother as a more important social problem and research subject than the unmarried father may help to explain this disproportionate ratio, as well as to provide a background for the data and discussion that follow. Thus: (1) The traditional double standard effects a harsher judgment of the female than of the male for sexual misbehavior during courtship, for involvement in an extra-marital affair, and for an unwanted pregnancy within marriage. (2) The "presumption of innocence until guilt is proven" provides far less protection for the unwed mother, whose maternity becomes self-evident, than it does for the unmarried father whose paternity must be proven. (3) The "principle of legitimacy" ² is maintained by censuring primarily the unwed mother; for since it is she whose evidence of sexual misbehavior overtly threatens the mores supporting legitimacy her behavior is censured as an object lesson to prevent these mores from "dying-out in the conscience of the society." (4) Public and research interest in a given social problem is in part a function of the financial burden which that problem imposes on taxpayers; and it is the unwed mother, rather than the unmarried father, who needs a maternity home, financial aid, and casework services. (5) With limited time and research funds, it is more economical to study groups of unwed mothers from maternity homes and welfare

² Cf. Kingsley Davis, "Illegitimacy and the Social Structure," *American Journal of Sociology*, 45 (August, 1939), pp. 215-233.

TABLE 1. AGE, EDUCATIONAL, AND OCCUPATIONAL DISTRIBUTION OF 201 WHITE UNMARRIED FATHERS

Age Groups	Per Cent	Educational Groups	Per Cent	Occupational Groups	Per Cent
15-18	15.9	College graduate	11.4	Professional and technical	12.4
19-22	22.4	Attended college	21.9	Business and managerial	7.0
23-26	18.4	High school graduate	33.8	Sales and services	11.4
27-30	12.9	Less than 12th grade	28.9	Skilled tradesman and craftsman	11.9
31-34	14.4	No data	4.0	Protective services	14.9
35 and over	5.5			Semi-skilled services	7.0
No data	10.5		100.0	Unskilled laborer	7.0
				Students	20.9
				Unemployed and no data	7.5
	100.0				100.0

agency files than to study *individual* unmarried fathers who remain anonymous in the population at large.

That these social attitudes and practices influence research on illegitimacy is evident in the present study of unmarried fathers. I did not obtain the data directly from them, nor did I ascertain their marital status—a datum recognized as crucial for defining unwed mothers, but overlooked in this study of their male counterparts.

POPULATION AND SAMPLE

Data on 736 out-of-wedlock births were obtained from three sources in Alameda County, California, throughout 1954: the Salvation Army Maternity Home, the Alameda County Hospital, and physicians' private practice.³ These 736 cases represent 3.2 per cent of the total live births,⁴ and the

three sample sources included all the discernible outlets for the delivery of babies during 1954 in Alameda County—a metropolitan area with an estimated population of 838,900 at the time of the study.

These 736 cases were reduced to 201. The 387 non-white unwed mothers were excluded because over four-fifths of them were attended at the county hospital where very few of the non-whites reported any data on unmarried fathers.⁵ Also excluded were 148 of the original 349 white unwed mothers (93 were divorced, separated, or married to a man other than the unmarried father; 23 did not report the identity of the unmarried father; 21 had had a previous out-of-wedlock pregnancy; six were pregnant as the result of rape or incest; and five had a Negro sexual mate). The sample for the present study, then, consists of the sexual mates of the 201 white unwed mothers who remained after these exclusions. An abbreviated socio-economic description of this sample is provided by the data in Table 1.

How representative of white unmarried fathers in general are these 201 sires described in Table 1? Unfortunately, survey data are not available on unmarried fathers in the general population, and data from regional

³ Since illegitimacy is not indicated on California birth certificates, the data were collected through the cooperation of the authorities and social case-workers at the Salvation Army Maternity Home and the Alameda County Hospitals. Eighty-nine per cent of the 546 physicians listed in the 1954 Alameda County Medical Directory provided data on the unwed mothers they attended in private practice. Questionnaires, face-sheets, and psychological tests were distributed at the beginning of 1954. Data were recorded as cases occurred and were returned to the writer at the end of the year.

⁴ This figure is lower than the estimated 4.4 per cent for the nation in 1954. When viewed separately for whites and non-whites, however, the Alameda County and national percentages for the former were identical—1.8 per cent of the total white live births. The percentages for the non-whites in Alameda County and the national estimate were 10.5 and 19.8 per cent, respectively. The lower non-white rate for Alameda County may reflect regional differences and the failure to correct for those not born in a

hospital. See U. S. Department of Health, Education, and Welfare, *Vital Statistics of the United States, 1955*, Washington: U. S. Government Printing Office, 1957, Table AN, p. LXXXVI.

⁵ The county hospital unwed mothers (84 per cent of whom were non-white) rarely gave advance notice to the hospital. An attempt to obtain data during their two or three days at the hospital proved unsuccessful, and the limited research budget precluded a post-hospital follow-up study.

TABLE 2. EDUCATION OF 193 UNMARRIED FATHERS AND 193 UNWED MOTHERS, AS REPORTED BY THE UNWED MOTHERS *

Unwed Mother's Education	Unmarried Father's Education			
	College Graduate (N=23)	Attended College (N=44)	High School Graduate (N=68)	Less than 12 years (N=58)
College graduate	69.6%	6.8%	1.5%
Attended college	17.4	72.7	5.9	1.7%
High school graduate	8.7	18.2	58.8	19.0
Less than 12 years	4.3	2.3	33.8	79.3
	100.0	100.0	100.0	100.0

* Eight of the 201 respondents did not report complete information on education.

or institutional studies are not reported by multiple sample sources and unmarried father-unwed mother pairs—as is done in the present study. Moreover, the temporary migration⁶ of unwed mothers during pregnancy makes them a tenuous basis for ascertaining the population source and representativeness of any given sample of unmarried fathers.

The lack of information on the distribution of unmarried fathers in the general population and the tendency to overgeneralize from "social-problems" research data justify the precaution of repeating in summary form the sampling procedure used in the present study. The *initial population* of 736 included all the unwed mothers reported as attended in a maternity home, the county hospital, and private practice, in Alameda County during 1954. The *sample* of 201 white unmarried fathers included the sexual mates of all the unwed mothers in this population who were white, single-never-married, pregnant for the first time, and who were not impregnated by a non-white, a relative, or by force. Thus, the data and interpretations presented below are not held to be applicable to unmarried fathers who are non-white, or who impregnate Negro, divorced, widowed, married, or recidivous unwed mothers.

⁶ The present study and the annual statistical reports from various maternity homes indicate that from 35 to 50 per cent of unwed mothers are non-residents of the county where they are attended during pregnancy. The unknown selective factors involved in this amount of temporary migration suggests the need for far more caution than is usually exercised in generalizing from existing data on unwed parents.

UNMARRIED FATHER-UNWED MOTHER MATING PATTERNS

The notion that unmarried fathers are much older and much better educated than the females they impregnate is not supported by the data in the present study. This notion has not been seriously challenged as long as descriptions of unwed mothers were derived primarily from institution-type samples and as long as descriptions of unmarried fathers were either unavailable or were not reported on an unmarried father-unwed mother pair basis. For the emphasis on the age and educational superiority of the unmarried father has received support from and represents the obverse of earlier conventional descriptions of unwed mothers. These descriptions, which emphasize the extreme youth, poverty, and lack of education of unwed mothers, were derived primarily from studies of unwed mothers in maternity homes, welfare agencies, and county hospital—where youth and low socio-economic status were socially imposed criteria for admission.⁷

Education. In Table 2, the educational attainment of the unmarried fathers is seen to be quite similar to that of the respective unwed mothers they impregnated. In Table 3, educational differences between the unmarried father-unwed mother pairs are seen to approximate educational differences between husband-wife pairs in a given census population.

⁷ See Clark E. Vincent, "The Unwed Mother and Sampling Bias," *American Sociological Review*, 19 (October, 1954), pp. 562-567, for a partial bibliography of these earlier studies.

TABLE 3. MEDIAN YEARS OF SCHOOL COMPLETED, FOR HUSBANDS AND WIVES WHO ENTERED FIRST MARRIAGES BETWEEN 1947 AND 1954, BY EDUCATIONAL LEVEL OF SPOUSE: UNITED STATES *

Educational Level of Spouse (Sexual Mate)	Median Years of School Completed			
	Husbands (Unmarried Fathers)	Wives (Unwed Mothers)		
Elementary	6.5	8.6	(6.0)	(8.7)
	8.8	9.6	(7.2)	(10.0)
High School	10.7	11.7	(10.6)	(10.6)
	12.4	12.4	(12.3)	(11.4)
College	13.9	12.7	(14.6)	(13.5)
	16.+	14.8	(17.+) (15.1)	
Total Group	12.1	12.2	(12.1)	(11.9)

* As compared with median years of school completed, for 193 unmarried fathers and 193 unwed mothers, by educational level of sexual mate. Adapted from Paul C. Glick and Hugh Carter, "Marriage Patterns and Educational Level," *American Sociological Review*, 23 (June, 1958), pp. 294-300, Table 3.

Age. Comparison of their ages with the ages of the respective unwed mothers they impregnated shows that 17 per cent of the 201 unmarried fathers were seven or more years older, 21 per cent were from four to six years older, and 56 per cent were within three years of the same age (six per cent did not provide information). These age differences would appear to approximate husband-wife age differences in the general population. Census reports do not permit rigorous comparisons of data on this point, but the following distribution was computed from data on 1,763,400 white wives between the ages of 14 and

22 who were married less than three years at the time of the 1950 census: 16 per cent of the husbands were seven or more years older than their wives, 31 per cent were at least four to six years older, and 53 per cent of the husbands were within three years of their wives' ages.⁸

The age data were then compiled as shown in Table 4. The unmarried fathers who had the greatest age seniority were found to be

⁸ Computed from 1950 U. S. Census of Population, *Special Report P-E, No. 2E*, Washington, D. C.: U. S. Government Printing Office, 1955, Table 3, pp. 2E-12, 2E-13.

TABLE 4. AGE AND EDUCATION OF UNWED MOTHERS BY AGE DIFFERENCES BETWEEN UNMARRIED FATHERS AND THEIR RESPECTIVE UNWED MOTHERS, AS REPORTED BY THE UNWED MOTHERS *

Unwed Mothers' Ages	Age Difference Categories			
	UF ¹ was 7 years older than UM ² (N=34)	UF was 4-6 years older than UM (N=33)	UF was 2-3 years older than UM (N=41)	UF was within 1 year of UM's age (N=70)
26 and older	17.6%	9.1%	2.5%	2.9%
25-22	55.9	48.5	7.3	2.9
21-18	17.6	27.3	63.4	41.3
17 and younger	8.9	15.1	26.8	52.9
	100.0	100.0	100.0	100.0
Unwed Mothers' Education				
	(N=33)	(N=32)	(N=39)	(N=68)
College graduate	48.5%	6.2%	5.1%	...
Attended college	36.4	50.0	18.0	4.4%
High school graduate	12.1	34.4	51.3	22.1
Less than 12 years	3.0	9.4	25.6	73.5
	100.0	100.0	100.0	100.0

* Age data for 23 and education data for 29 cases were not reported.

¹ UF = unmarried father.

² UM = unwed mother.

mated predominantly with older or with college-educated unwed mothers, while those with the least age seniority were mated predominantly with the youngest unwed mothers or with those who had not completed high school.

"SEXUAL EXPLOITER" AS AN EX POST
FACTO LABEL

The following discussion has some of the characteristics of a strawman argument, inasmuch as it is based on an assertion for which evidence is not provided. The assertion is that the label of "sexual exploiter" has frequently been affixed to unmarried fathers and used implicitly, at least, as a partial explanation of the "cause" of illegitimacy. No attempt is made to substantiate this assertion, since space limitations preclude citing the several hundred references from among which it was derived and since selectively quoting from only a few such sources⁹ is a misleading substitute for the systematic study (content analysis or attitude survey) needed to confirm or to negate adequately the assertion. Moreover, the primary intention of the present discussion is to clarify the normative content of the label, rather than to examine the extent of its usage.

The term sexual exploiter (or its conceptual equivalent) is interpreted here as representing an ex post facto label for unmarried fathers—a label that is affixed *after* the female has become pregnant. This inter-

pretation is derived from the foregoing data, which fail to show any overwhelming or one-sided age and educational superiority of the unmarried fathers over the unwed mothers in the present study, as well as from additional questionnaire data for over 500 unwed mothers and from individual interviews and counseling sessions with 82 unwed mothers and 32 unmarried fathers (to be reported in a separate paper).

That this term is an ex post facto label in many if not the majority of cases in which it is applied becomes more understandable when it is viewed within a normative framework; for the notion that unmarried fathers tend to mate sexually with and thereby exploit females who are much younger, less-educated, and poorer, is in one sense a logical projection of folkways and attitudes which form a part of mate-selection patterns in general. For example, the traditional view of the male as being somewhat superior or dominant in male-female relationships is still reflected in the tendency of females to marry males who in comparison with themselves are taller, older, have more education, and possess the potential for higher occupational status. Thus, the same age, educational, and socio-economic superiority of the male over the female that is accepted and encouraged as normative in dating, courtship, and marriage is viewed as the basis for and as evidence of sexual exploitation when illegitimacy is involved.

Any "exploitation" present in the relationship during the time when illicit sexual unions are occurring is as likely to be reciprocal as one-sided. This relationship has some of the characteristics of an informal and implicit contract, and it is usually only after one party loses too much that the other party is viewed as an exploiter.* The unwed mother is as likely as the unmarried father to be "exploiting" during the time their relationship is active, but with a different focus. She tends to use *sex* (or sexual enticements in the larger sense) as a means—to dates, companionship, an expense account, upward mobility, and possible marriage. He reverses this. He tends to date, provide companion-

⁹ Leontine Young, for example, gives the following summary of the public's attitudes concerning the unmarried father: "In short it was more or less taken for granted that he was in any case a pretty worthless character probably without scruples or conscience, from whom little could be expected, and that little to be exacted for the most part only by compulsion." (*Out of Wedlock, op. cit.*, p. 131). Young also notes that there is "no possibility of knowing what the unmarried father is like as a person, . . . until this accusing and punitive attitude toward him is abandoned." (*Ibid.*, p. 133). Although she does not deal extensively with socioeconomic data, Young does emphasize the reciprocal, emotional exploitation existing between unmarried fathers and unwed mothers. "As we have come to know something of the unmarried father as an individual, one factor has emerged clearly. He is in almost every case a counterpart of the neurotic personality of the unmarried mother" (*Ibid.*, p. 134).

* For a more extensive use of "market" concepts in the analysis of certain relations between men and women, see William J. Goode, "Illegitimacy in the Caribbean Social Structure," pp. 21-30, of this issue of the *Review*.—The Editor.

ship, pay the bills, court, and express love—as a means to sex.

This is not to say that they view each other or sex in any deliberate or even conscious manner as representing only a means, nor is it to say that sexual experience is the main motivation of and focus for their relationship. Rather, it is to state (and thereby overemphasize) only one of many orientations, and only one of many points on a means-end continuum, in male-female relationships. Given this limited context, to say that the unwed mother tends to use sex as a means to other goals is only to explicate what is implied when being erotically attractive and enticing (short of acquiescence, if unmarried) receives normative sanction as a part of female role-expectations in general, as evidence of femininity, and as a "typically feminine" means of achieving a variety of goals. Within this same context, to say that the unmarried father tends to use dating as a means to sex is but to isolate (and thereby oversimplify) one of many implications of current male role-expectations. For when masculinity is a socially sanctioned goal for the male and when the pursuance of sexual favors is sanctioned as evidence (means) of masculinity, dating will represent one of the potential means to the goal of sexual experience which, in turn, is a means (evidence) to the goal of demonstrated masculinity.

Consistent with these normative sanctions, is the fact that the label of "exploiter" is rarely applied to the male involved when the female's skill and performance in her expected role result in marriage—even though the male is considerably older, better educated, and of higher socio-economic status than the female. (In fact, the female is sometimes thought to have "exploited" her sexual means when a premarital pregnancy is associated with her marriage to a male of much higher socio-economic status). There is also little use of the label in a relationship in which sexual intercourse occurs without pregnancy and without marriage—presumably, because each of the parties has incurred either some benefit or no serious loss from the temporary relationship.

It is when pregnancy without marriage occurs that he is labeled the exploiter, she the exploited. His superiority in age, educa-

tion, and socio-economic status, which would have been viewed as evidence of her desirability and as a rightful reward for her feminine skills if marriage had occurred, is now pointed to as evidence of his exploitative position—of which he took advantage.

TWO DOUBLE-STANDARDS IN SEXUAL BEHAVIOR

Thus, we come full-circle with reference to mores associated with not one but two double-standards in sexual behavior: females are judged more harshly than males for sexual misbehavior. An illicit pregnancy is judged more harshly than is an illicit sexual union.

As to the first double-standard, the harsher judgment of the female can be understood within the following context: mores which ascribe to the female the traditional role of "maintainer and upholder of morality;" mores which sanction feminine skill in sexual enticement; and mores which serve to define the female's sexual misbehavior as containing the potential for a more serious social problem (unwed motherhood) than is represented by the male's sexual misbehavior (unmarried fatherhood). The softer judgment of the male is related to his traditional role of pursuing sexual favors as evidence of his masculinity, his lack of evidence of sexual misbehavior that would overtly threaten cherished family values, and social practices and attitudes that preclude his identification as a burden on taxpayers.

The second double-standard, the tendency to judge the *result* (illicit pregnancy) more harshly than the *cause* (illicit sexual union), is illustrated by such folk-sayings as "what they don't know won't hurt them" and "out of sight, out of mind." The unwed mother's changing profile makes her sexual misbehavior public knowledge, thereby threatening to weaken traditional sex mores if left uncensured; whereas, the single female who remains discreet and non-pregnant while engaging in illicit sexual unions poses no overt threat to these mores. Also, as noted above, the financial costs of an illicit pregnancy pose a very tangible problem for public taxpayers, while (with the exception of V. D. Clinics) the "costs" of an illicit sexual union usually remain the private problem of the partici-

pants. Thus, the harsher judgment of an illicit pregnancy than of an illicit sexual union is in large part a function of the greater normative and financial threats which the former represents. Censure of the unwed mother and her behavior is one of the oldest methods used to decrease these threats.

It is at this point that humanitarian values come to the fore to soften the censure of the unwed mother by labeling the unmarried father as an exploiter. She now can be seen more as a violated victim and less as a violator. Diverting some of the censure from her to the unmarried father is also a way of rewarding her for reinforcing that aspect of traditional motherhood which places the welfare of the unborn child ahead of the fears and solitariness of the mother.

Within this context the *ex post facto* labeling of the unmarried father as an exploiter is less a censuring of his *sexual misbehavior* (since it is primarily the female's responsibility to avoid illicit sexual unions), and more a censuring of his *role misbehavior*—his failure to assume the traditional masculine role of protecting the female. Having illicit sexual relations and becoming pregnant are primarily the female's "areas of fault." Failing to protect and to shield her from shame (by marrying her) when she becomes pregnant is primarily the male's "area of fault."

In this sense the male passes the "masculinity test" when he pursues and obtains sexual favors from the female, but fails the test when he refuses to protect her from shame and to provide for her when she becomes pregnant. The female tends to fail one aspect of the "femininity test" when she acquiesces to an illicit sexual union, but passes another aspect of this test when she accepts the role of motherhood and completes her pregnancy in a traditionally heroic manner—alone and faced with censure. In so doing she partially diverts the censure from herself to the missing father who traditionally should protect her and provide for the baby.

Current efforts to censure and to counter this weakening of the traditional protective

role of the male consist primarily of name-calling—affixing the *ex post facto* label of sexual exploiter to the unmarried father. Although such efforts may appear to be weak deterrents to the breakdown of the traditional protective role of the male, they are consistent with the mores noted at the beginning of this paper and consistent with the current disclarity concerning male and female roles in general.

SUMMARY AND IMPLICATIONS

On the basis of the present study of white unmarried father-unwed mother pairs, whose educational and age differences approximate those sanctioned in dating and mate-selection patterns in general, the term sexual exploiter is interpreted as an *ex post facto* label for the unmarried father.

The significance of this interpretation is not as an explanation of the cause of illegitimacy, but as a clarification of the mores from which the label is derived and to which it gives support. Within the larger normative setting, this *ex post facto* label serves humane values by modifying the blame or censure of unwed mothers who lend support to the heroic aspect of the traditional role of motherhood by bringing their pregnancies to fruition. This label also serves to sanction the traditional protective role of the male by censuring males who fail to protect females from shame, and serves to support cherished family values by censuring any father who leaves a mother in an unwed status whereby she overtly threatens the traditional family system.

This interpretation of sexual exploiter as primarily an *ex post facto* label can also serve to broaden the base for research in illegitimacy, inasmuch as it suggests the need to examine the following matters: the study of unmarried parents by pairs; the reciprocal exploitation involved in illicit sexual unions that do not result in pregnancy; and the differential goals involved in, and the social practices that provide normative sanction for, the instrumental use of illicit sexual relations.

PREMARITAL CHARACTERISTICS OF THE RELIGIOUSLY INTERMARRIED IN AN URBAN AREA *

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A sample of persons who married outside their faith is compared with a sample of intra-married respondents on a series of premarital variables. In general, the intermarried are found to report a lesser early tie to religion, greater dissatisfaction with early relationships with parents, greater strife in family of orientation, lesser early family integration, and greater emancipation from parents at time of marriage. Each of these differences is interpreted as representing a way in which barriers to intermarriage are removed. Differences were also found on such variables as religion, socio-economic status, and ethnicity. These differences are at least partially explicable in terms of group differences on the first set of variables.

SEVERAL investigators have compared the premarital characteristics of people who marry within their faith with those of people who intermarry.¹ In general, these studies have been focussed on a single religious group and they have been limited to variables such as religious training, generation, and socio-economic status. The present

paper also compares the intermarried with the intramarried, but it differs from the previous research in that its main interest lies in early patterns of interaction and in that it contains data on respondents from each of the three major religious groups.

It is well known that almost all religious groups in this country oppose interfaith marriage, and it appears probable, though the data are far from adequate, that a majority of the general population shares this opposition.² These facts lie at the source of our interest in the premarital characteristics of the intermarried. From a study of these characteristics we hope to learn how it was possible for them to intermarry despite what seems to be general disapproval of such marriages. We want to know, in effect, how these people surmounted the usual barriers to intermarriage. The problem, then, is one of social control.

* Condensed version of a part of the author's unpublished doctoral thesis, "Interfaith Marriage in an Urban Area," Indiana University, 1958. I should like to express my gratitude to Leo Srole and Thomas S. Langner of the Midtown Mental Health Project for their kindness in making available to me the data upon which this study is based. The Midtown study was conducted by the Department of Psychiatry of the New York Hospital (Payne Whitney Psychiatric Clinic)-Cornell University Medical College. The study was instigated and developed by the late Thomas A. C. Rennie; it is currently directed by Alexander H. Leighton as part of the Cornell program in social psychiatry. Thanks are also due Clifford Kirkpatrick and Sheldon Stryker of Indiana University for their many helpful suggestions.

¹ Among the studies in this area are Reuben R. Resnick, "Some Sociological Aspects of Inter-marriages of Jews and Non-Jews," *Social Forces*, 12 (October, 1933), pp. 94-102; J. S. Slotkin, "Jewish-Gentile Intermarriage in Chicago," *American Sociological Review*, 7 (February, 1942), pp. 34-39; Gerald J. Schnepf, *Leakage from a Catholic Parish*, Washington, D.C.: Catholic University Press, 1942; John L. Thomas, "The Factor of Religion in the Selection of Marriage Mates," *American Sociological Review*, 16 (August, 1951), pp. 487-491; Claris E. Silcox and Galen M. Fisher, *Catholics, Jews, and Protestants*, New York: Harper, 1934; M. C. Elmer, *The Sociology of the Family*, Boston: Ginn, 1945, p. 195; Robert O. Blood, *Anticipating Your Marriage*, Glencoe, Ill.: Free Press, pp. 38-41. An extensive survey of the field, particularly in regard to studies of incidence, appears in Milton L. Barrow, *People Who Intermarry*, Syracuse: Syracuse University Press, 1946.

² There are no data on the attitudes of adults, but several studies report on the attitudes of students. The results of these studies vary greatly. A study of college students in Florida reports that about 90 per cent of them would not be willing to consider a marriage outside their faith. A study of a national sample of high school students, on the other hand, reports that only 20 per cent of the Protestants would not consider marrying a Catholic and 25 per cent of the Catholics would not consider a Protestant as a mate. Other data show opinion to be fairly evenly divided. See, Victor Christopherson and J. Walters, "Concerning Marriage and Family Life," *Sociology and Social Research*, 43 (September-October, 1958), pp. 16-22; Hadley Cantril and Mildred Strunk, *Public Opinion, 1935-1946*, Princeton: Princeton University Press, 1951, p. 431; Judson T. Landis, "Marriages of Mixed and Non-Mixed Religious Faith," *American Sociological Review*, 14 (June, 1949), pp. 401-407; Thomas, *op. cit.*

Generally, culturally proscribed behavior is prevented by both internal and external controls. The former operate as attitudes which lead individuals to believe that the proscribed behavior is wrong. Because of the desire for self esteem and for the reduction of anxiety, these internal controls, if they exist, can usually prevent undesired behavior. No society, however, depends entirely upon internal controls. Invariably, there are external pressures helping to bring about conformity to social norms, which take a variety of forms from the threat of the loss of esteem to the threat of death.³

In regard to marriage, both types of mechanisms of social control (referred to below as barriers) are operating to prevent heterogamy. Typically, people have instilled in them attitudes which lead them to oppose interfaith marriage, and these attitudes prevent many such marriages. In addition, external pressures prevent interfaith marriage among people who are not particularly opposed to it. The sanction is usually the possibility that the approval of significant others will be lost.

These controls obviously have not worked in the case of the intermarried; the problem of this research is to determine why. It is assumed that the ultimate source of barriers to interfaith marriage in American society lie in the family of orientation and formal religious organizations. Therefore, if one wishes to discover why the controls were ineffective in certain instances, it seems appropriate to investigate the characteristics of the respondent's parents and the nature of his relationships with his family and religion. If, in the case of an intermarried respondent, it can be shown that these sources did not produce effective barriers to intermarriage, we will consider the marriage to be explained.

METHOD AND SAMPLE

Within this framework, six general hypotheses were drawn up. Each took the form: people who have characteristic A are more likely to intermarry than are those

with the opposing characteristic A'. The rationale for each hypothesis is that possession of the characteristic attributed to the intermarried is thought to be indicative of the probable absence of one or more of the barriers to intermarriage.

The available data did not permit a direct test of the general hypotheses and, therefore, in each case, a series of specific, testable, hypotheses was formulated. Support for the specific hypotheses will be taken as evidence of the validity of the general hypotheses.

The data to test the predictions come from interviews of 1,167 people, between the ages of 20 and 59, who lived in the "Midtown" section of Manhattan. This sample was selected from the 1,660 cases studied by the Midtown Mental Health Project. The present sample does not include the 493 respondents in the original sample who had never married nor the 55 whose marriages are not classifiable as interfaith or intrafaith.

The Midtown sample was selected by means of a three stage area-probability sample design. Blocks, houses, and dwellings were the units used, and within each dwelling a single respondent was chosen randomly. Eighty-seven per cent of those so chosen were successfully interviewed; the non-respondents do not seem to differ in relevant respects from those who did cooperate.⁴ Comparisons between the sample and census data indicate that the former is representative of the Midtown area. The population of Midtown is representative of the white population in Manhattan.⁵

Respondents were classified according to type of marriage on the basis of the religions in which they and their spouse were reared. A marriage was considered to be an intermarriage when the line between the three principal faiths was crossed. Table 1 shows the distribution in the sample on the basis of this definition.

There are 304 intermarried respondents and 863 intramarried. If categories are com-

⁴ Some demographic data were obtained for the non-respondents. No significant differences appeared when these materials were compared with the obtained sample and with the Midtown population.

⁵ Further information on the sampling procedure may be found in Thomas A. C. Rennie, Leo Srole, and Thomas S. Langner, *Midtown, Manhattan: The Mental Health Story*, forthcoming.

³ This traditional sociological conception of social sanctions has been recently discussed in Melford E. Spiro, *Children of the Kibbutz*, Cambridge: Harvard University Press, 1958, pp. 299-322.

TABLE 1. DISTRIBUTION OF SPOUSE'S EARLY RELIGION BY RESPONDENT'S EARLY RELIGION

Respondent's Religion	Spouse's Early Religion								
	Catholic		Protestant		Jewish		E.O. ¹		Total
	No.	Per Cent	No.	Per Cent	No.	Per Cent	No.	Per Cent	No. Per Cent
Catholic	469	78.6	107	17.9	14	2.3	7	1.2	597 100.0
Protestant	110	28.3	257	66.1	18	4.6	4	1.2	389 100.2
Jewish	14	8.9	15	9.5	129	81.6	—	—	158 100.0
E.O. ¹ or other	11	47.8	3	13.0	1	4.3	8	34.8	23 99.9
Total	604		382		162		19		1167

¹ Eastern Orthodox.

bined, Catholics have an intermarriage rate of 21.4 and Jews a rate of 18.4 per cent. Both of these are considerably below the Protestant rate of 33.9 per cent. It may also be noted that when Christians intermarry they tend to marry other Christians rather than Jews. The incidence of Jewish-Catholic marriages is about equal to that of Jewish-Protestant unions.⁶

HYPOTHESES AND FINDINGS

Since we are interested in clues concerning the ways in which barriers to interfaith marriage were removed, it seems appropriate first to consider the religious background of the respondent. One of the main sources of anti-intermarriage attitudes is to be found in the teachings of the various religions. If the parents of the respondent were not religious, he is unlikely to have been effectively exposed to these teachings and, therefore, unlikely to have developed the attitudinal bar to interfaith marriage.

Similarly, one of the important external barriers is the opposition of parents. If the parents were not religious, they would be less likely to oppose the intermarriage of their child. Thus, one of the main sources of anti-intermarriage pressures would be removed. If we further assume that the children of secular parents will probably not be religious, the pressures of the churches are also unlikely to be effective bars to intermarriage.

On the basis of this reasoning the first general hypothesis was formulated: *Those*

*whose parents had a weak tie to religion are more likely to intermarry than are those whose parents had a strong tie to religion.*⁷ Three specific hypotheses were formulated to test the validity of this first general hypothesis. It was predicted that a significantly higher percentage of the intermarried than of the intramarried will report that: (1) religion had no importance to their parents; (2) their parents never attended church; (3) the respondents themselves did not attend parochial school.

The data which test these hypotheses are found in table 2.⁸ Without religion controlled, all of the specific hypotheses receive support. In general, then, the intermarried are more likely to have had secular parents. In the three religious groups the differences are not always large enough to be statistically significant, although, with one exception, they are in the predicted direction. But this factor explains very little of the intermarriage, for even among the intermarried very few had a secular background.

The intermarriage of some of the respondents nevertheless is explained. They were freed from the barriers to intermarriage by the fact that their parents were not religious. Since the first general hypothesis is supported, we shall remove from further con-

⁷ Though not stated specifically, the predictions made in all hypotheses are thought to hold for each religious group as well as for the sample as a whole.

⁸ The hypotheses are tested by means of a one-tailed *z* test. A *p* of .05 or below is used as the criterion of significance. The *p* values must be taken as approximations, for the assumptions of the *z* test are not completely met. Of particular importance is the fact that a complex sampling design is used; this generally increases the number of significant differences obtained.

⁶ The range in the incidence of interfaith marriage is very great. The rates found in this study do not seem to be unusual. See Barron, *op. cit.*, pp. 166, 174, 177.

TABLE 2. SIGNIFICANCE OF THE DIFFERENCES BETWEEN INTERMARRIED AND INTRAMARRIED RESPONDENTS BY RESPONDENT'S EARLY RELIGION

Hypothesis	Catholic	Protestant	Jewish	Total
I. Parents of Intermarried Less Tied to Religion.				
1. Higher percentage of intermarried report religion not at all important to both parents.	<.001 ¹	>.10	>.25(—) ²	<.05
2. Higher percentage of intermarried report parents never attended church.	>.25	<.05	>.05	.01
3. Higher percentage of intermarried did not attend parochial school.	.02			
II. Intermarried More Likely to Report Dissatisfaction with Early Relationships with Parents.				
1. Higher percentage of intermarried have a high D Score.	<.05	>.10	>.25	.02
III. Intermarried More Likely to Report Strifeful Family Interaction When Young.				
1. Higher percentage of intermarried have a high C Score.	<.05	.10	>.25(—)	.02
2. Higher percentage of intermarried report they argued with parents over religion.	<.001	>.25	>.10	<.01
3. Higher percentage of intermarried report that they often disagreed with their parents.	<.05	>.25(—)	<.25	>.25
4. Higher percentage of intermarried report their parents quarreled often.	.01	>.25	>.10	.10
5. Higher percentage of intermarried report their parents were divorced, separated, etc.	<.25	>.10	.05(—)	>.25
IV. Intermarried More Likely to Report Tenuous Ties to Family When Young.				
1. Higher percentage of intermarried have a low I Score.	.05	.05	.01	.001
2. Lower percentage of intermarried report that they did not live with both their real parents up until the age of sixteen.	<.25	>.25	<.05	.06
3. Lower percentage of intermarried report grandparents lived with them.	<.05	.02	.02	.001
4. Lower percentage of intermarried saw none of their relatives.	.25	>.25	<.05	.05
V. Intermarried More Likely to have been Emancipated from Parents at Time of Marriage.				
1. Higher percentage of intermarried were over 30 at time of marriage.	.05	>.05(—)	<.05	>.10
2. Higher percentage of intermarried were married previously.	<.001	.06	>.05	<.001
VI. Intermarried More Likely to Report Parents Had a "Difficult Time."				
1. Higher percentage of intermarried report parents were in poor health.	>.05	.02	.10(—)	.02
2. Higher percentage of intermarried report parents had a hard time making ends meet.	>.05	>.05	>.25	.25
3. Lower percentage of intermarried report parents had no problems.	>.10	>.10	>.25	>.10
4. Higher percentage of intermarried report parents were the "worrying type."	>.05	>.10	.25(—)	.25

¹ Unless otherwise noted the p is based upon a one-tailed z test.² A negative sign in parentheses indicates that the difference is not in the predicted direction.

TABLE 2.—Continued

Hypothesis	Catholic	Protestant	Jewish	Total
VII. Miscellaneous Variables ^a				
1. Birth Order	<.50	<.30	<.10	<.05
2. Father's education	<.001	<.05	<.50	<.10
3. Father's SES	<.001	<.10	<.30	<.20
4. Respondent's education	<.001	<.01	<.50	<.50
5. Educational mobility	<.05	>.50	>.50	<.50
6. Rural-urban childhood	<.01	>.50	— ⁴	<.50
7. Generation	<.10	<.20	>.50	<.20
8. Ethnicity	<.001	—	—	<.001

^a In each of the miscellaneous variables the chi-square test was used, and the P refers to a two-tailed test. The directions of the obtained differences are described in the text.

⁴ A dash indicates that the cell frequencies are too small to allow calculation of a meaningful chi-square.

sideration all respondents who reported that religion had no importance to their parents and that their parents never attended church. Thus, the hypotheses which follow refer only to persons who had at least one religious parent. This limitation is necessary because these hypotheses refer to relationships with parents, and for our concerns such relationships have different meaning depending upon whether or not the parents were religious.⁹

The second and third general hypotheses refer to the respondents' early relationships with their parents. It is reasoned that respondents who say they had unsatisfactory relationships with their parents are less likely to consider parental wishes when they choose a mate; this would represent the removal of certain external pressures. Moreover, dissatisfaction in a religious home may encourage attitudes favorable to intermarriage or, at least, reduce bias against such marriages. If this is the case, there is also a removal of a barrier at the attitudinal level.

The second general hypothesis follows from this reasoning: *Those who were dissatisfied with their early relationships with their parents are more likely to intermarry than are those who were satisfied.* One specific hypothesis was formulated to test this general hypothesis. A score of early dissatisfaction with parents (D Score) was constructed. It is hypothesized that a signifi-

cantly higher percentage of the intermarried will receive high scores.¹⁰

For the sample as a whole and for Catholics the hypothesis is supported. In the Protestant and Jewish groups the differences are not large enough to be significant.

The third general hypothesis states: *Those whose early family life was strife-filled are more likely to intermarry than are those who came from families in which interaction was relatively harmonious.* The primary test of this hypothesis is based upon the Intra-familial Conflict (C) Score,¹¹ but additional data, including material from two items used in the C Score, are also presented. It is predicted that a higher percentage of the intermarried will: (1) receive a high score on the

¹⁰ Since the concerns of this research were not foreseen when the original study was designed, the available materials were insufficient to allow the construction of true scales. In several cases, however, a group of questions seemed to have a common referent, and these were combined into crude indices which may be called "simple scores." These scores indicate the number of apparently related questions that were answered in a given direction. The deficiencies of such scores are, of course, numerous, and they should be taken as first approximations. For the D Score a series of seven statements of criticism of parents was used. The score refers to the number of statements accepted by the respondent as descriptive of his feelings about his parents when he was young. For a list of the questions and a comparison of the marriage types on each question, see Heiss, *op. cit.*, pp. 49-50.

¹¹ Five questions were used in the C Score. The questions dealt with in specific hypotheses (3) and (4) were included with three questions designed to determine the respondent's attitude to family strife. For example: "As a child, how much were you afraid of family quarrels?" *Ibid.*, p. 64.

⁹ After removal of these cases the sample numbered 1,118—284 intermarried and 834 intramarried.

C Score; (2) report disagreement with their parents about religion when they were young; (3) report that they often disagreed with their parents when they were teenagers; (4) report that their parents quarreled often; (5) report that there was divorce, separation, or desertion in their family of orientation.

The data show that there was more strife in the families of intermarried Catholics than in those of the intramarried Catholics. A similar relationship, though not as strong, holds for the sample as a whole. The Protestant and Jewish groups, however, do not show many differences on these variables. In fact, there is a significant difference opposing the hypothesis: families of orientation broken by divorce, separation, or desertion were reported more frequently by intramarried Jews than by intermarried Jews.

The fourth and fifth general hypotheses are concerned with the strength of family ties early in life and at the time of marriage. It is assumed that pressures from parents are ineffective when family ties are weak. In such cases parental desires would probably be of little moment when a mate choice is made, and this probably would remove an important external barrier. One of these factors may also be relevant in terms of attitudes. When early family ties are not close, inculcation of anti-intermarriage attitudes is probably less effective than in cases in which the family is tightly knit. This reasoning forms the basis of the fourth and fifth general hypotheses.

The fourth general hypothesis states: *Those who had only tenuous ties to their immediate and extended families when they were young are more likely to intermarry than are those whose early family ties were close.* Data from scores of early family integration, the I Score,¹² serve to test this hypothesis. It is predicted (1) that a higher percentage of the intermarried will receive a low score on this index. It is also predicted that a smaller percentage of the intermarried will report that: (2) they lived with both their real parents until the age of sixteen; (3) when they were young their grand-

parents lived with them; and (4) they saw their relatives fairly often.

The I Scores provide strong support for the fourth general hypothesis with reference to all religious groups. Comparison of the marriage types on several of the items which make up this score also support the hypothesis. It may be noted that the presence of relatives in the home is not related to intermarriage rates unless these relatives were grandparents, in which case there is a strong relationship.

The fifth general hypothesis states: *Those who are emancipated from parental influence at time of marriage are more likely to intermarry than are those who are not.* The data contain only two indices of probable emancipation from parents at time of marriage. It is predicted that a significantly higher percentage of the intermarried (1) married for the first time at a late age;¹³ and (2) have been married previously.

Both specific hypotheses seem to apply to the Catholic group. In the other groups there are substantial differences in the predicted direction on the second hypothesis, but the data on Protestants do not show the expected relationship relating to age at marriage.

The relationship between the final general hypothesis and the basic concern of this paper is not too clear-cut. This hypothesis refers to parental problems when the respondent was young because it is thought that a generally difficult home situation may help to weaken the hold of parents upon the child. Thus, the following general hypothesis: *Those whose parents had a "difficult time" are more likely to intermarry than others.* In this case, the specific hypotheses predict that a significantly higher percentage of the intermarried than of the intramarried will report that: (1) their parents were in poor health; (2) their parents had a hard time "making ends meet;" (3) their parents had problems; and (4) their parents were the "worrying type."

¹³ The validity of this item as an index of the general hypothesis is open to question. People who marry late in life may do so because of an over-attachment to parents. Evidence of the latter, however, was found in very few of the cases of late marriage. In these few instances, relatively advanced age at marriage is not considered an adequate explanation of the intermarriage.

¹² The I Score is composed of the questions dealt with in specific hypotheses (2), (3), and (4) and a fourth question indicative of degree of attachment to the home. *Ibid.*, pp. 71-72.

This sixth general hypothesis does not receive adequate support from the data concerning any of the groups. Only one of the specific variables produced any significant differences.

The analysis of the six general hypotheses suggested a corollary to the first general hypothesis. If a secular background represents a weakening of barriers to interfaith marriage so too should a family of orientation in which the parents were intermarried.¹⁴ It was predicted, therefore, that the children of intermarried parents have a higher intermarriage rate than those of intramarried parents. The results are consistent with the prediction, yielding a *P* of .06 for the sample as a whole.

The marriage groups were also compared on a series of miscellaneous variables. The first of these involves consideration of the intermarriage rates of persons occupying different birth order positions. Although the differences are not always large enough to be significant, the findings show considerable consistency. In all of the religious groups those persons who were the youngest in their families have the highest intermarriage rates, only children the next highest, and eldest children the lowest. The "other" birth position group shows no consistent ranking, but the rates of these respondents tend to approximate the group rate.

Three other variables which were considered—father's education, father's socio-economic status, and respondent's education—are all taken as indices of the respondent's socio-economic status. There are significant differences between the marriage types in the Protestant and Catholic groups on all three of these variables. While the data are not completely consistent, the trend is in a different direction in each group. Among the Catholics there is a positive relationship between status and intermarriage rates, but the Protestant group shows an inverse relationship. Data for the Jews indicate no clear-cut trends.

A further variable that was studied is educational mobility. Here the only significant

difference was found among the Catholics. Upwardly mobile Catholics have a higher intermarriage rate than the group rate. This difference, however, is probably more a reflection of high education than social mobility *per se*.

There appears to be no relationship between rural or urban origin and interfaith marriage rates. Only one significant difference was found—farm-reared Catholics have lower intermarriage rates than other Catholics. This is probably due to the fact that many of these Catholics are foreign-born.

Except for the low intermarriage rates among first-generation Catholics, the comparison by generations produced no significant differences. The total group show low rates for the first and third generations and high rates for the second and fourth generations, but the differences are not very large.

Differences in nationality background of considerable size were noted. Among the Catholics, the United States group,¹⁵ Germans, Russians, and "Others" have high intermarriage rates. All other Catholic nationality groups show rates below the rate for the Catholics as a whole. The Jews are represented, in adequate numbers, by three nationality groupings: Germans, with a high rate of intermarriage, and Hungarians and "Russians, etc.," each with a low rate.

SUMMARY AND DISCUSSION

The data, as they bear upon the six general hypotheses, are summarized in Table 3. For the sample as a whole, five of the hypotheses receive what is considered to be adequate support.¹⁶ In general, the intermarried as compared with the intramarried are characterized by: (a) non-religious parents, (b) greater dissatisfaction with parents when young, (c) greater early family strife, (d) less early family integration, and (e) greater emancipation from parents at time of marriage.

The Catholic data support these five hypotheses.

¹⁵ Persons who are at least fourth-generation Americans were classified in the United States group.

¹⁶ The evaluations of the general hypotheses are informal. They are based primarily upon comparisons of the number of significant differences obtained with the number expected by chance alone.

¹⁴ The reasoning here is as follows: Intermarried parents are not likely to instill in their children anti-intermarriage attitudes unless the marriage is a very unhappy one. Similarly, intermarried parents are not likely to oppose the intermarriage of their children.

TABLE 3. EVALUATION OF GENERAL HYPOTHESES BY RESPONDENT'S EARLY RELIGION

Hypothesis	Catholic	Protestant	Jewish	Total
I. Parents of Intermarried Less Tied to Religion.	*	*	-	**
II. Intermarried More Likely to Report Dissatisfaction with Early Relationships with Parents.	**	-	-	**
III. Intermarried More Likely to Report Strife Family Interaction When Young.	**	-	-	*
IV. Intermarried More Likely to Report Tenuous Ties to Family When Young.	*	*	**	**
V. Intermarried More Likely to Have Been Emancipated from Parents at Time of Marriage.	**	-	*	*
VI. Intermarried More Likely to Report Parents Had a "Difficult Time."	-	-	-	-

A dash indicates that the hypothesis is not adequately supported. An asterisk means that the hypothesis receives some support, and two asterisks signifies strong support.

In the Protestant group, though the differences are generally in the predicted direction, in many instances they are not large enough to be significant. Only two of the general hypotheses receive substantial support: intermarried Protestants had relatively weak ties to family and to religion.

Most of the general hypotheses do not seem to apply to the Jewish group. Several of the differences are not in the predicted direction, and few of the specific hypotheses are supported at the .05 level. The data suggest that intermarried Jews differ from intramarried Jews only in the strength of their family ties—while young and at the time of marriage.

One may view these findings in another way. The hypothesis concerning early family integration appears to hold for all groups. The data for two of the three religious groups and for the sample as a whole support the hypotheses concerning emancipation at time of marriage and the parents' religiosity. Only data from the Catholic and "Total" categories support the hypotheses related to dissatisfaction with parents and early family strife. The prediction that the parents of the intermarried had a "more difficult time" is not borne out.

It is clear that not all of the ways in which the barriers to interfaith marriage may be removed have been isolated. This is particularly the case with respect to the Protestant and Jewish groups, where only two of these ways

have been found.¹⁷ This shortcoming also holds for the Catholics. Some of the intermarried Catholics have none of the characteristics considered necessary for intermarriage. In all groups certain respondents intermarried despite the apparent presence of the bars to intermarriage discussed in this paper.

The failure to account for all cases of intermarriage has two main sources. First, certain relevant considerations have not been isolated; the data do not show all the ways in which the potential barriers to intermarriage may be removed. Second, since it was necessary to use fallible indices of the factors that were isolated, in some cases the "bars were down," though it was not apparent.

A deviant case analysis is indicated at this point, but no additional data are available for that purpose. It is necessary, therefore, to substitute speculation about other ways in which bars to intermarriage may be removed.

It has been assumed that devotedly religious, intramarried parents oppose the in-

¹⁷ A full discussion of why certain of the factors seem to represent the removal of barriers in some religious groups but not in others is not attempted here. This finding, however, does not seem too surprising. These variables, of course, may have different meanings in different religious groups. For example, the barrier to intermarriage among Jews is probably more generally cultural than merely religious. Jews from secular backgrounds may indeed be exposed to anti-intermarriage attitudes and pressures.

termarriage of their children. Under certain circumstances this might not be the case. For example, some parents are committed to the idea that choice of a mate should be left entirely to the child; they probably would not apply pressure in opposition to intermarriage. In some cases, one would suppose, the acceptability of the intended spouse as a person would overcome objections on religious grounds. In addition to other possibilities, the conversion of the spouse or, among Catholics, the use of the Church's sanctioning procedure for mixed marriages might make them at least acceptable to the parents. Consideration of such possibilities suggest that the present analysis has somewhat overemphasized the degree of parental opposition to interfaith marriage. It is believed, however, that in many of the unexplained cases opposition was present, and that we have as yet not explained how it was overcome.

It is very likely that we have isolated only a few of the ways in which a person can be sufficiently emancipated from his parents so that their objections to his intermarriage will not be effective. For one thing, the parents may have been deceased when the marriage took place; our data do not include this factor. A sociologically more significant possibility is that the respondent was strongly convinced that choice of a mate is a personal affair and possessed the psychological maturity to back up his belief.

The paragraphs above suggest some further ways in which respondents may have been freed from *external* pressures opposing intermarriage. What about internal barriers? At the level of attitudes, the following possibilities are illustrative: (1) rejection of religion by the respondent would probably be associated with a rejection of anti-intermar-

riage attitudes even if the latter were once accepted; (2) commitment to a democratic ideology may be stronger than anti-intermarriage attitudes; (3) unwilling but deep involvement may result in intermarriage despite unfavorable attitudes toward it.¹⁸

The findings from the tests of the general hypotheses have proven of value in the explanation of the differences on the miscellaneous variables discussed above. To give only one example, the high intermarriage rates of college educated Catholics as compared with Catholics with only elementary schooling could have been predicted because the former more frequently reported secular background, intermarried parents, low family integration, and previous marriage. But in some instances not all the variables provide a basis for prediction, and sometimes the direction of the prediction is not clear for certain factors point one way and others in another. In no case, however, is an incorrect prediction clearly indicated. This may be taken as further evidence that it has been possible to isolate some, but not all, of the factors relevant for understanding interfaith marriage.

Clearly, much additional work remains to be done in this area. It would be valuable if future research could be designed so that it would not have to depend upon reports of attitudes that were held in the dim past. Although such reports in the present analysis seem to be reliable, there is no way to test their validity. Longitudinal studies would be exceedingly difficult, of course, but reports of objective factors relating to childhood rather than attitudinal material probably would enhance such studies.

¹⁸ These matters are considered in more detail in Heiss, *op. cit.* pp. 118-119.

THE IMPACT OF RECRUITMENT ON THE ORGANIZATION OF THE LARGE LAW FIRM *

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Interviews were conducted with 188 lawyers from 21 law firms, each employing 50 or more lawyers. The evidence indicates that the recruitment complex which at once must satisfy both the demands of the firm and the recruit is being severely strained by increased competition. In order to meet this competition changes have been made in the organization of law firms. These alterations can take place in any part of an organization, for in reality, although special recruitment apparatus has been set up, the whole system is part of the recruitment mechanism. The planned changes, however, often produced side effects which have gone beyond the desired goal.

COMPETITION for the services of a "special kind" of lawyer has forced the large law firms¹ to modify their structure in order to compete more effectively with each other and with other agencies seeking similar legal talent. This paper analyzes (1) some of the personnel requirements for the legal staff of the large law firm, (2) the changes in environment which influence recruitment mechanisms and processes, and (3) the way these factors have resulted in adaptive organizational change. It thus provides examples of some aspects of structural alteration in an organization—an area where illustrations are sorely needed.² In addition, the study provides information concerning side effects of planned organizational change.

It is hoped that the following discussion carries meaning beyond the understanding

of some structural changes in law firms. The functioning and perhaps the survival of social systems in general depend on their personnel. This, in turn, as in the law office, is contingent on how well a system is organized to recruit and maintain its supply of manpower. The recruitment mechanism is not a separate or distinct department or organ of a social system. Any feature of a social system, for example, conditions of job security or an organization's reputation for fair dealing, may enhance or depress its attractiveness to the prospective member and from this point of view may be considered an aspect of the recruitment system or mechanism. Any part of an organization, then, may be important to the recruitment process.

Alterations in the environment, in the available labor market, say, or in the competitive situation, or changes in the functions of an organization requiring personal qualities or skills or both which are different from those the system currently recruits, may result in changes in a social system. Since so much depends upon the nature of personnel, both the failure of a system to recruit individuals capable of carrying on its activities and the difficulty or insecurity in obtaining such personnel are serious sources of strain to which the system may be highly sensitive. Organizational changes are often made to reduce such strain. Of course, adaptive changes of this kind occur not only in law firms but in other social units.

METHOD

The data for this paper were obtained from a number of sources, among them semi-

* Revision of a paper read at the annual meeting of the American Sociological Society, August, 1958. The research was supported by a grant from the Graduate School at Indiana University and by a Fellowship in Law and the Behavioral Sciences at the University of Chicago Law School. This paper deals with a small portion of a larger study of the Wall Street lawyer and the organization of large law firms. I am indebted to Albert K. Cohen for his numerous suggestions.

¹ Only law firms with 50 or more lawyers were considered large. Eighteen of the 21 law firms with 50 or more lawyers in New York City are included in this study. The largest firm had 125 lawyers connected with it. Three firms located outside New York were also studied in order to test the importance of location on the organization of law offices.

² For an excellent historical analysis of the effect of recruitment, see Sigmund Diamond, "From Organization to Society: Virginia in the Seventeenth Century," *American Journal of Sociology*, 53 March, 1958, pp. 457-475.

structured interviews, averaging two hours, with 188 lawyers from 21 different large law firms. Placement officers of the law schools at Chicago, Columbia, Harvard, Yale, and New York Universities were also interviewed and, where possible, their office records were examined. In addition, some senior law students from these schools were asked about their job preferences. A simple thematic content analysis was undertaken of job announcements sent to Harvard Law School. Finally, an analysis was made of the background characteristics of all the partners (468) of 20 large law firms in New York City.³

PERSONNEL REQUIREMENTS

The large law offices and especially the giants of Wall Street have, as one associate put it, "... an inflated idea of what they want. They're looking for well rounded men who are law review."⁴ Actually they want more than that. They want men who also have pleasing personalities, are from the "right" schools with the "right" social backgrounds, have a "clean-cut" appearance, and are endowed with tremendous stamina. A former dean of a law school states:

To get a job they [students] should be long enough on family connections, long enough on ability or long enough on personality, or a combination of these. Something called acceptability is made up of the sum of its parts. If a man has any one of these things, he could get a job—if he has two of them, he can have a choice of jobs—if he has three, he could go anywhere. . . .

The large law firms prefer the man with all three attributes: lineage, ability, and personality. What they want, what they need, and what they get are related, but are not necessarily the same thing. The large law firms recruit manpower in order to do their job, provide continuity to the firms, and keep existing performance up to the clients' expectations and to the lawyers' own high standards. It is not essential for the operation of a firm that all of their lawyers have all

three attributes. This is so if only because the firms have developed a hierarchical system of practice which allows the best and most experienced lawyers to supervise those less able and less experienced. The firms' actual needs, in fact, include lawyers who can and are willing to do routine work. The firms' recruitment policies, moreover, are generally geared to what is wanted and not to what is absolutely needed. The senior men are accustomed to dealing with able and energetic people and would be impatient with others. In addition, seeing themselves as an elite and viewing their work as highly important, they want to perpetuate this image. Thus there is keen competition for the preferred lawyer—the personable man from one of the select eastern law schools, who graduated with honors from an Ivy League college, and was at the top of his law school class. That high academic attainment in law school is important in recruitment is noted in a Harvard Law School report which states that many firms request "as a qualification that a man have 'B or better' grades."⁵ Confirmation of this point is found in the breakdown of the job activities since graduation of the Yale Law School graduating classes for 1955, 1956, and 1957, which reveal that 53 per cent of the Yale men the large law firms in New York City employed came from the top quarter of their classes, and that 27 per cent were from the first decile.

The large firms want and obtain men from the elite eastern law schools. Seventy-four per cent of the sample graduated from Harvard, Yale, and Columbia law schools. A survey of the 468 partners listed in the *Martindale-Hubbell Law Directory* of 1957 from 20 large New York City firms shows that 71 per cent of them were graduates of Harvard, Yale, and Columbia. This figure is of special importance since almost all men who become partners (employers) in large

³ Data for this analysis were derived from the 1957 issue of the *Martindale-Hubbell Law Directory*, Rahway, N. J.: Quinn and Boden, 1957, Vol. II.

⁴ Law review or law journal men are law students who run the legal journals and who usually are at the top academically of their class.

⁵ *Harvard Law School Placement Information* (a leaflet published at Harvard), September, 1956, p. 3. Walter S. Carter is credited, "as far back as 1860," with initiating the recruiting system adopted by the larger law firms. It was his practice to pick the best students from the law schools and take them into his office. See Otto E. Koegel, *Walter S. Carter, Collector of Young Masters*, New York: Round Table Press, 1955, pp. x, 3, 8.

law offices begin their careers as associates (employees) fresh from law school. Significantly, also, 32 per cent of these men are listed, or belong to families who are listed, in the Social Register.

CHANGES IN THE ENVIRONMENT

While the large firms obtain some of the men they want, each firm wants a greater proportion of these men.⁶ The increasing competition for these elitists initially stems from the increasing amount of practice going to this kind of firm. This expansion of legal work is generally attributed to the continuing growth of big business and to the multiplication of laws affecting commerce.⁷ Riesman also suggests that "it takes more people to do the same amount of work and that ever higher standards of conspicuous production go into the definition of the standard of work—Parkinson's law, in other words."⁸ Personal observation suggests that while Wall Street lawyers work very hard today, they probably do not work as hard (as indicated by historical sources) as their predecessors did in the past—another reason why more lawyers are needed to do the job now.⁹

The growing number of large law firms throughout the country is perhaps of more

importance. One author, by examining the legal directories published up to 1900, found that most firms were very small (by current standards) during the nineteenth century.¹⁰ Today, in addition to the 21 large offices in New York City, there are seventeen large law firms operating in other section of the country; in 1949 only five of the seventeen could have boasted of 50 or more lawyers. These firms along with most of the other large legal organizations have grown in size. Available figures for the population of lawyers for fifteen of these large firms located outside of New York indicate that in 1959 about 347 more lawyers were connected with these organizations than in 1949. This increase discloses only a portion of the call for lawyers, for during the 1949-1959 decade some lawyers left the firms and other law school graduates had to be found to replace them. These firms look for the same kind of lawyers as do the more numerous Wall Street firms. Forty-one per cent of the lawyers from one large California law office graduated from Harvard and Yale law schools. Generally, however, (and this is particularly true of Texas firms though not of the largest Massachusetts firm) large offices outside of New York are more willing to hire lawyers from schools other than the eastern elite institutions.

The turnover in the New York firms is sizable, often more than ten men a year for the larger offices. This occurs in part because of the departure of misfits and malcontents and in part because a number of Wall Street firms have recently adopted an "up-or-out" policy: if a firm decides that an associate is not going to be made a partner, he is informed of the decision so that he may either look for another job or accept one found for him by the firm. This policy, plus the insistent demands made by the firms corporate clients for lawyers to work in their organizations, creates a constant call for preferred lawyers. Thus the usual demand for such lawyers is increased by the departure of more associates from the law firms. In

⁶ It is not possible in this paper to present extensive evidence indicating the increased demand for the preferred lawyer. Much of the material for this assertion is to be found in the detailed interviews upon which a good portion of this study is based. To supplement these interviews and the available literature, in an effort to further assure accuracy, copies of the manuscript were sent to lawyers (including some managing partners and associate deans of law schools) involved with large law firms. They were asked to comment on the verity of the data. None took exception to the above statement.

⁷ See, e.g., J. D. Wright, "The Lawyer's Role in Modern Industry," *Western Reserve Law Review*, 9 (September, 1958), pp. 425-426.

⁸ Letter from David Riesman, September 8, 1958.

⁹ Henry W. Taft in *A Century and a Half At the New York Bar* (New York: privately printed, 1938, p. 40) quotes Mr. Strong, a member of a firm which eventually became the large Wall Street firm of Cadwalader, Wickersham, and Taft, and who in 1824 wrote: "During the whole winter I have been confined in the office from about 8 in the morning till about 10 at night and at work as busily as a man in harvest." See also David Riesman, "Law and Sociology: Recruitment, Training and Collegueship," *Stanford Law Review*, 9 (July, 1957), p. 666.

¹⁰ Edwin C. Austin, "Some Comments on Large Law Firms," *The Practical Lawyer*, 3 (April, 1957), p. 9.

¹¹ Wright finds that private industry employed 15,063 full-time practicing lawyers in 1954, 3,789 more than they did in 1950. *Op. cit.*, p. 427.

addition, there is initial recruiting competition for the preferred lawyer by government agencies, the courts, corporations, and universities.

Competition is complicated by the proportional reduction in admissions to the Bar in the United States. The rate of admission in 1930 was 81 per million population, in 1958 only 57.¹² It is further complicated by the large law firms' preference for men from the Ivy League eastern schools and the fact that these institutions have had no significant rise in the number of registrants,¹³ and by the firms' reluctance to hire women¹⁴ and lawyers from other "minority" groups. This reluctance is breaking down, probably because of changes in the general atmosphere concerning minority groups—and because of increasing competition. The large firms also seek men from the midwest, though not in any great numbers; but the graduates from the midwestern schools seem not to have the same predilection as do students from eastern elite schools to join large law firms.

While most large law firms are now employing Jewish lawyers as associates, they probably limit the number of Jews they will accept. Judge Proskauer, a senior partner in a major "Jewish" firm, in 1949, wrote:

... It did not take me many days to discover that the doors of most New York law offices in 1899 were closed with rare exceptions, to a young Jewish lawyer. Fifty years have elapsed since then and I am happy to record that there has been a distinct improvement in the situation; though it still remains true that gen-

erally the Jewish student must qualify twice for such employment.¹⁵

This continuing, though impressively modified restriction against Jewish lawyers, cuts down the number of potential recruits who meet at least the high academic requirements demanded by the large firms. That this seems to be the case is reinforced by the estimates of law school officials who indicate that the proportions of Jewish students attending Ivy League law schools has increased. Their estimates are borne out in some measure by a B'nai B'rith report of a 54 per cent increase in Jewish enrollments in Ivy League colleges between 1946 and 1955,¹⁶ such undergraduate training is a prelude perhaps to matriculation in Ivy League law schools. In addition, the number of Jews at the top of their graduating classes is large. At Yale Law School, for example, it was estimated by contributors to the *Law Journal* that 63 per cent of the members of the journal for 1955-1956 and 1956-1957 were Jewish. By eliminating some Jews from consideration the firms cut down their potential supply of academically superior individuals.

WHAT LAW SCHOOL GRADUATES WANT

What the large law firms want has been described; what they can obtain depends not only on the available supply and competitive conditions but on the relationships between what the firms have to offer and what the potential recruits themselves want. The law school senior thinks about where he wants to go, what he wants to do, and how he wants to practice.

Location of the job is often mentioned as a consideration, the West being frequently cited as a preference. The evidence indicates, however, that if the fledgling lawyer believes that opportunity lies in New York City and the East instead of San Francisco and the West, he will go East. Harvard Law School records, for example, show that of the 105 Harvard men hired by the large law firms

¹² Ross L. Malone, "Lawyers—Supply and Demand," *Trusts and Estates*, 98 (March, 1959), p. 186.

¹³ Comparison of the combined law school registration of Harvard, Yale, and Columbia for 1948 and 1958 indicates a gain of only 151 students, an increase of five per cent. However, the 1958 registrants represent a decrease of 550 students from the year 1947. These figures are based on data compiled by John G. Hervey; see "Law School Registration, 1949," *Journal of Legal Education*, 2 (Winter, 1949), pp. 218, 220-221, and "Law School Registration, 1958," *Journal of Legal Education*, 11 (2, 1958), pp. 259, 263, 266.

¹⁴ Women lawyers are not discussed in this paper. There are only eighteen in New York City who list themselves as working in large New York Law firms in a directory of women lawyers. Dorothy Thomas, editor, *Women Lawyers in the United States*, New York: Scarecrow Press, 1957.

¹⁵ Joseph M. Proskauer, *A Segment of My Times*, New York: Farrar, Straus and Young, 1950, p. 30.

¹⁶ Robert Shostack, *The Jewish College Student*, Washington, D. C.: B'nai B'rith Vocational Service, 1957, p. 37.

in New York City for a selected three-year period, 21 stated that they had hoped to obtain positions outside of New York City.

Many law seniors have no particular preferences as to fields of the law. These undecided constituted the largest number in the 1957 graduating class at Harvard.¹⁷ While the rest of the class did indicate preferences, the desired fields of work named by most seniors often differ from later choices. This was true for members of the sample. Again, the larger opportunity rather than a specific branch of the law seems to be the more important element influencing job choice. As the market becomes more competitive, however, the neophyte can become more selective, and therefore the ability of the firm to offer the location and specialty the potential recruit prefers becomes correspondingly more important.

Perhaps of greater significance to the young lawyer is the setting in which he chooses to practice. A major decision the job seeker from the eastern elite schools must make is whether or not to practice in a large law firm. Many students, of course, especially from the eastern schools, desire to practice in large law firms. This is evidenced by the many uninvited but welcome applicants for jobs in large law firms each year. It is understandable that these offices should draw a considerable number of men, for they offer prestige, good starting salaries, and a potentially high future income; they also handle the largest and probably the most interesting corporate matters and are at the center of the business community's power structure. But many other students have clearly decided against the large firm for such reasons as the following: "I don't want to get lost in those law factories." "They make you specialize too soon." "I don't care for that kind of impersonal practice; I want to help people with their problems." "The work is too routine." "You're not your own boss." "You don't get enough responsibility." "The work is too hard." "You have to wait too long to become a partner—you can move up faster in a small firm." "I want to have some time with my family."

¹⁷ *Harvard Law School Placement Information, op. cit.*, p. 2. Examination of other information bulletins reveals some variation in the order of preference.

"You don't see clients or learn how to get them."

Many lawyers who finally enter the large law firms hold attitudes about the firms shared by those who do not enter them. These reluctant lawyers look for an organization where they will not feel as lost, will not have to specialize as quickly, and where they will do more responsible and interesting work. In addition to this desire to modify the negative aspects of work in the large law firm, some men, explain their choice of a particular organization by citing available postgraduate education, a chance to get ahead quickly, the opportunity to deal with "big matters," work with interesting people, or several of these reasons.

Law students, however, often base their job choices according to placement officers and the evidence obtained from interviews with seniors and individuals in the sample, on personal bias, half truths, myths, fads, and other factors not normally considered important in making such decisions. The recruiter must take these factors into account in a tight job market.

The most frequently heard "reputational myth" involves the notion that one or two firms work much harder and much longer than other large firms. There are legends about the kinds of clients of a firm; there is the rumor that certain offices will not hire female or Jewish lawyers. Often a firm is given a "short-term halo" based on the report of a returning alumnus. The decision to join a certain firm sometimes depends upon how a man "feels" about the organization. Thus one associate reporting on a position he had refused: "While they offered me a job, I didn't feel they wanted me;" and a student respondent speaking of a partner who had just interviewed him: "He's the nicest fellow I've ever met." To the anxious recruit the hiring partner represents a symbol of things to come.

IMAGE-MAKING MACHINERY

It is to these wishes and notions that law firms must cater and that, when appropriate, they try to manipulate or change. In a competitive market, with its scarcity of preferred men, students can be selective. Under these circumstances, the firms first sell the idea of practicing in the large organizations to the

reluctant or ambivalent recruit. (The Harvard Law School Association of New York City holds a smoker each Christmas holiday for Harvard Law School job hunters to which they invite speakers who present the case for employment in large law firms, small firms, and corporations.) The firms then try to sell themselves individually, hoping that these devices will maximize their chances to attract the preferred type of men.

To change, maintain, or create attractive images, and to assure or reassure the recruit, the firms send notices to the placement offices of the law schools in which they plead innocent to certain detrimental charges and claim desirable attributes. This is part of their "image-making machinery." In order to determine the content of these notices, announcements sent to Harvard Law School were analyzed. Examples from these bulletins illustrate the main themes: Most of them are designed to bolster a desire to defeat a fear.

One fear law students have about the large firm is that they will be forced to specialize before they know what they really wish to do. Some firms belittle this anxiety by assuring that they are "... not as rigidly departmentalized as some very large offices," or by reporting that "In so far as possible, law clerks are given an opportunity to work on problems in those fields in which they are most interested and are not required to select a specialty or confine their work to one field." Hand in hand with qualms about early specialization is the anxiety about not receiving "proper training." Attempts are made to allay both of these fears. One announcement reads:

Men coming with the firm directly out of law school are not assigned to a department of the office until they have been with the firm for one or two years. During this period conscious effort is made to see that these men have the opportunity to work with as many different partners in as many different fields of the law as possible. Emphasis is placed on broad general experience. This will include not only research work and preparation of memoranda of law but also drafting legal papers, participation in conferences and with clients, attendance at court hearings, several weeks' experience in the managing clerks' department and generally a month with the New York legal aid society.

Some firms announce that they do not use the pool system (through which a lawyer can

be assigned to do anything for anybody), hoping that this statement will help to blunt the senior's fear that he will "get lost" in the large organization. To assure him of his independence, one firm declares: "It is the purpose of our training to bring a man to the point where he can work independently as quickly as he is able to." To assure recruits of security, firms announce that they do not place men from the outside over old associates; or that they receive requests from corporations for lawyers, so that if individuals are not admitted to partnerships their future nevertheless is assured.

These promotional notices to the law schools make a difference, but their effect is limited. In addition to announcing opportunities, they may weaken the resolve of some men against the large law firm and strengthen favorable predisposition of others. Mainly they make claims for their firms and assist in creating an image. Over a period of time, however, such promotional activities can have only limited influence. For if the claims are not implemented by commitments which correspond to the reality of the organizational experience, disappointed personnel and a seriously damaged public image spread by disaffected personnel are the result. In the long run, then, the credibility of the firms' claims depends to a great extent on their readiness to back up their statements by corresponding changes in organizational structure and functioning.

ORGANIZATIONAL ADAPTATION

Competition for lawyers among the large law firms in New York City is limited in two major ways: the firms will not pirate an employee from another law office; and they maintain a gentlemen's agreement to pay the same beginning salary, commonly called the "going rate."¹⁸ However, although these

¹⁸ A small number of cases have been reported in which these agreements were not strictly adhered to.

The going rate in 1958 was 6,500 dollars for a beginning lawyer; it was 4,000 dollars in 1953. Thus, while the major firms agree on salary, they must take into consideration competition from other sources. The firms do compete, however, by raising psychic, as well as other kinds of relatively intangible, "income."

Another indication of increased competition is found in the reports that some firms are beginning to "wine and dine" prospective employees, using

agreements limit salary competition somewhat, they intensify competition in other respects. Most of these firms are old and conservative and proud of both these attributes, and therefore impose upon themselves further limits as to what they will do to attract legal talent. Nonetheless, it is necessary for immediate and long-run survival to recruit "proper" legal talent. To do this, changes in the organization of the law firms have been made to meet the demands of the recruit. This has occurred despite the fact that there is resistance to such change, especially on the part of the older, more conservative partners. These men, who often seem to think of themselves as akin to small-town general practitioners, do not like the notion of a segmented, departmentalized, hierarchical organization, regarding it as something not quite professional. They also resent the time reorganization may take away from the practice of the law and make changes only when they persistently believe that they must. When changes do take place they do not happen quickly.¹⁹

Law firms no longer believe that they can afford to wait until the candidate comes to them: more and more, firms are sending lawyers to visit the major schools to look over the crop for the next year. Harvard, for example, was visited in 1955-1956 by representatives of 185 law firms and corporations, and in 1956-1957 by 194 prospective employers. This practice, and the tradition of law offices of seeing anyone who wants a job, have led to the formalization of the role of "hiring partner," who is generally one of the most attractive and personable of the partners. Creation of this position is an early attempt by a firm to put its best foot forward. The hiring partner is needed not only

to attract graduates but to save the valuable time of other partners. While visiting the various law schools, the hiring partner weeds out the poorer prospects and invites the better ones to visit the firm in New York. The hiring partner is also more able than his colleagues to evaluate the candidates since he sees a good percentage of all applicants. In addition to the men invited to visit firms, each year between the Thanksgiving and Christmas holidays there is a mass migration of other young lawyers to Wall Street.

Some firms—in self defense but also to maintain good will and a good reputation at the law schools and with the legal profession, while at the same time continuing their work and recruitment—have set up elaborate systems to take care of the estimated 300 to 400 candidates seen during the year. One large firm developed the following system:

Any lawyer who applies for a position is interviewed, no one being turned away. Ten to twelve associates are designated as interviewers. The receptionist tries to spread the work around. Each associate rates the candidate as a "one," "two," or "three." If rated "one," the candidate is sent to a partner who sits on the hiring committee. If rated "two," the applicant may be "all right" but the associate does not think him acceptable; but he is also sent to a partner—though generally he is given less time. A "three" rating means that the applicant is rejected; even rejected candidates, however, are sent to a partner, though not necessarily one who is a member of the hiring committee. The partner spends five minutes with a "three" and then sends him on his way—but he tries to leave the applicant with a favorable impression of the firm. If the partner disagrees with the associate, he refers the candidate to a member of the hiring committee, where each man rated as "one" is sent immediately. At least three people on the hiring committee see this highly rated applicant. If the candidate passes this test, then the hiring partners have him see other partners.

Not all firms have such an elaborate procedure. One law office assigns fourteen partners to duty, three different ones each day, who do the screening. This procedure reflects the view that better public relations are maintained if partners do the initial interviewing rather than associates. If one of these partners passes favorably on an applicant, he is sent to two or more partners. Eventually he is interviewed by the hiring partner, who sees all candidates and compares them, a

recruiting techniques of big business. This is not only an index of competition; it also irritates many lawyers who must resort to this procedure, even though in only a limited way, for they feel it is not "professional."

Competition for the preferred lawyer is further pointed up by the willingness of law firms to hire some seniors, although they are scheduled to go into the armed forces before they can practice, in order to insure the firm of their services after they are discharged.

¹⁹ Cf. Emily P. Dodge, "Evolution of a City Law Office, Part 1: Office Organization," *Wisconsin Law Review*, 2 (March, 1955), p. 182.

process which requires valuable time; but this expenditure is thought to be necessary and well worthwhile.

Another device of the large law firms to further selective recruitment is to invite second-year law students to clerk with them during the summer. Most firms do not really need these "summer boarders" as workers, but their value is threefold: the boarders get to know the firm; they provide the firm with a preview of their ability—if they are good and are liked, they are offered jobs when they graduate; in any event, they return to the law schools and report what they have seen. Spending some time being pleasant to the summer boarders and at times designating partners to look after them during their temporary stay, these clerks, it is hoped, will help to paint a favorable picture of the firms.

When a firm makes a major revision in its organization because of recruitment, it does so to meet the similar demands of a number of candidates. Generally such a change is further justified because it also satisfies other internal requirements. In some instances the alterations are made primarily to satisfy other demands and secondarily as a recruitment device.

One request most applicants make of law firms is that they provide opportunities for training. This request is closely related to the seniors' fear that they will be forced into quick specialization. Candidates are told that they will be provided diversity in their work, and the firms try to do this. Usually this plan is frustrated by the fact that an associate who has been successful in one kind of work becomes known as "good" or "expert" in that particular area of law; more of his colleagues then send him work in his now acclaimed "specialty," so that soon he becomes an expert in fact. Thus, it is costly for the firm to change his functions, and partners who have been relieved of this particular job and hence save time and energy, are reluctant to take it on again. Consequently, the associate remains in his special area and does not get the broad training he desires. More and more firms, however, finding themselves committed to training their new men (because it is a good recruiting device, to be sure, but also because many partners think that it helps to make better

lawyers and therefore benefits the firm), are beginning to formalize further their educational programs. In a sense these firms are becoming postgraduate vocational schools.

One of the largest law partnerships is ahead of the others in this respect. The hiring partner in this office, who had devised the educational program, found that it was attracting candidates and was becoming known in law schools and throughout the "street." This formalized program, designed to provide for varied experience, is described by its sponsor as follows:

We have a rotating system. An associate has to spend a portion of three years in three different departments. I keep a chart of where they've been and how long they stay and when they've been in a department long enough I move them. We found that unless you have regular notices that the man is changing departments you don't move the man properly. If you do it without the records and without the formality, you find that a man is never free and the people involved in the problem have forgotten about rotating.

This firm decided that this program had to be installed if they were going to give good training. Despite this, it still meets with individual resistance. A partner will say "I just get a man trained and you take him away from me." Some of the partners said that we aren't running a school. They feel, however, that eventually they will get good lawyers. The associates like it and that's one reason why I get such good results in hiring. We do a lot of things for our associates. We even send them to special courses, like tax courses.

As they debate the merits of large firms, recruits often complain that "you don't get ahead fast enough—it takes forever to make partner." Some firms assign the title "Junior Partner" to their senior associates, perhaps creating the illusion that the associates are moving up faster than they actually are—and this illusion may aid the firm in its recruitment efforts. Many firms have initiated rules and other changes to reassure ambitious recruits. For example, candidates fear competition from relatives of partners, and members of a firm may fear the possibility of taking a colleague's unqualified son into the firm as a partner. Hence the increasing adoptions of a rule against nepotism.

Anxiety about being lost in the giant firm is often mentioned by both the applicant and the young associate. The pool system, although it functions formally as a training

device, adds to that concern. In addition, many associates do not feel that this plan for work assignment is "professional." Because of the disapproval of the assignment of bright young lawyers to more or less mechanical jobs, some firms are doing away with the pool system.

The "up-or-out rule" is designed to provide that lawyers who are not going to be made partners leave the firm and so insure a constant flow of new talent into the organization. This rule also has the side effect of counteracting the complaint that "you never know how you're doing," it requires that eventually (usually within a ten-year period) associates be informed that they are not going to be partners. Thus the rule guarantees a steady turnover in the law firms. One consequence is that some offices function as employment centers in order to give security to their associates, to demonstrate their own view that it is not "professional" (or at least not "nice") to fire a lawyer, and to provide their corporate clients with good legal and executive talent. Generally the managing partner solicits jobs and suggests associates from his firm to fill them. Some offices cultivate this function more than others and can provide excellent job opportunities for their future alumni. While not every man can become a partner, very few leave the firm without a good position—sometimes one paying a higher salary than the missed partnership itself. The firms which best provide employment outlets win the reputation for "taking care of their men."

AN ADDITIONAL TYPE OF ADJUSTMENT

During World War II many large firms began to accept Jewish lawyers as associates at an accelerated rate. These Jews were usually in the top of their classes in the Ivy League colleges and law schools, and the firms, initially at any rate, hired them partly because competition made it difficult always to secure the lawyers they preferred. Probably it will become more of a problem, however, for law firms to continue to recruit Jewish lawyers if they do not promote some of them to partnerships. This is especially the case at the present time, since some of these lawyers are approaching completion of the approximately ten-year maturation pe-

riod during which partnerships are decided. The American Jewish Congress survey of lawyers' employment experience reported in 1954 that there already existed some reluctance on the part of Jews to apply for certain jobs: "Over one-third [35 per cent] of the Jewish respondents [from the 1951 graduating class of Chicago, Columbia, Harvard and Yale law schools] reported that they refrained from making application to certain law firms and business houses because of a belief that these firms used discriminatory employment practices. . . ." ²⁰ More recently one respondent declared that "the reason we don't have more Jews [in the firm] is that too many of them feel that they won't become partners and leave. Some won't even take a job with the firm."

Jewish lawyers appointed as partners provide models of success for Jewish candidates, demonstrating that it is possible for them to become partners. Certain New York offices have already promoted some of their Jewish associates to partnerships. (A few firms in the past have included Jewish partners, but usually these were "German" Jews whose families had been in this country for generations, and many of them have had important business connections.) Most respondents believe that the other firms will soon follow this example. This change in policy not only represents new unofficial rules but may alter the social complexion and perhaps the "family" and club-like atmosphere of the law office. Nonetheless, even "white shoe" firms ²¹ are now recruiting Jewish lawyers, and these firms also will find, if they wish to hire the bright Jewish law review editor, that it is good recruitment policy to provide models of success.

SIDE EFFECTS RESULTING FROM ORGANIZATIONAL CHANGE

Changes designed to aid recruitment affect the organization beyond the desired goal, and

²⁰ American Jewish Congress, Commission on Law and Social Action, *A Survey of the Employment Experiences of Law School Graduates of Chicago, Columbia, Harvard, and Yale Universities*, New York: mimeographed, 1954, p. 2.

²¹ Firms, like the schools themselves, are sometimes called "white shoe" when they are composed in large part of lawyers from Ivy League schools with Social Register backgrounds.

each alteration produces side effects. Formalization of the position of hiring or assignment partner, for example, means that the lawyers occupying this position assume new managerial duties and are compelled to curtail their practice of the law. The development of these managerial posts also signifies that the younger partners who usually fill them must be given enough authority to carry out their roles. This necessity results in a slight shift in the power relations within the firms.

In some instances, what began as a small alteration have snowballed into something of major importance. Many firms, as noted above, now promise the recruit job security—either within the firm or elsewhere. It was not too difficult to fulfill this promise until some law offices instituted the “up-or-out rule;” but then the increased placement needs required a great deal of work. It is important that the firms satisfy both their corporate clients and their associates, since their activities in these areas are closely tied in with their ability to recruit and to place more men. The relatively minor task of finding jobs for some of their associates with corporate clients has grown to large proportions; one firm estimates that it has as many as 800 alumni.

Similarly, the promise made by law offices to provide advanced training for their recruits has led some firms to formalize their educational procedures. Among the side effects of this change is the development of additional managerial and tutorial duties and the loss of some measure of freedom by partners, for it is now difficult for them to keep men they have personally trained and whom they like.

Other side effects have been noted. Tasks which were meant to be temporary or minor tend to become fixed—for example, running employment bureaus, recruiting at law schools, taking in summer boarders. Almost all such changes call for new rules and further integration on the part of the law firms.

SUMMARY AND DISCUSSION

The recruitment standards of the large law firm are high, and they are hard to meet because competition for preferred men is strong and increasing. Therefore, law school

graduates who fulfill the high standards can be more discriminating than formerly in their choice of jobs. The law offices try to satisfy some of the demands of the preferred graduates in a number of different ways, and not all firms have adopted all methods.

The development of “image-making machinery,” however, is an adaptation made by all of the large firms. Most offices also have formalized the role of “hiring partner.” Other changes include provisions for training, speedier advancement into the rank of junior or limited partner, “up-or-out” rules, regulations against nepotism, and reduction of discrimination against the employment of Jews and women.

These alterations are designed partly to help the firms meet the manpower problems, and the adaptations constitute changes in the recruitment mechanism. This mechanism includes any aspect of the structure or functioning of the system that affects the intake and loss of personnel. Therefore, changes in the mechanism refer not only to the recruitment process in the narrow and commonly understood sense but also to alterations in the firm which affect its attracting and holding power. These alterations can take place in any part of an organization, for in reality the whole system is part of the recruitment mechanism.

Organizational changes to meet the demands of growing competition for preferred lawyers often produce unanticipated consequences, mainly in the direction of increased bureaucratization. This tendency is indicated by the development of formal rules and formal stratification and the formalization of training duties.²²

Some changes and side effects of planned change lead to new strains and dilemmas. Concerning the function of the law firm: “Are we a law firm or a school?” On the employment of women and Jews: “What will our clients think?” “What will it do to our little family?” In satisfying short-term needs as against possible long-term benefits: “If we inaugurate an ‘up-or-out’ policy we will be losing men who know their job and are valu-

²² While these features are not in themselves equivalent to bureaucracy they are among its defining characteristics. The relationship of bureaucracy to the legal profession will be given fuller treatment in another publication.

able to the firm." On the use of corporation techniques of recruitment: "Is it professional?"—There are other problems, but the most interesting development has come about because the best students and young associates wish to be treated in what they consider to be a more professional manner—they want more independence and responsibility—and the firms needing these young lawyers set up

systems of rules to protect them from the severity of impersonal organization. The young associate receives short-run protection; the firm in the long-run becomes more bureaucratic. The law firms soon may be facing another and perhaps more important problem growing out of the need for further bureaucratic procedure at the possible cost of a less professional staff.

CULTURAL OCCASIONS AND GROUP STRUCTURES: A PHOTOGRAPHIC ANALYSIS OF AMERICAN SOCIAL SITUATIONS

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Photographic data are utilized to study the relationship between cultural occasions and plurality structures in more than 1,000 social situations in the United States. New typologies for the classification of cultural occasions and of group structures are developed for the study and subjected to a test of reliability. Results indicate that significant structural differences are associated with differing cultural occasions. Structural features investigated include the size of the plurality and the distribution of its members in the discrete structural-functional units of the spatial situation.

CULTURE has been defined as a "set of ready-made definitions of the situation which each participant only slightly retails in his own idiomatic way,"¹ and as "a set of blueprints for action."² Culture so conceived is a force producing standardized behavioral responses to situational stimuli.

The literature contains convincing demonstrations that such standardizations of response do occur.³ There seem, however, to be

few systematic studies of the relationship between culture and social structure, particularly those investigating the situations of smaller groups. Among the few who have attempted this type of investigation are Rose and Felton, who designed an experiment in which the relationships between changing group membership and the cultural processes of invention, habit, and borrowing were investigated.⁴ The present study has a somewhat different focus, undertaking a quantitative analysis of the ways in which human groups are structured in various types of cultural occasions. Photographic data, the scientific possibilities of which have only recently been indicated, are utilized.⁵

¹ Clyde Kluckhohn and William H. Kelly, "The Concept of Culture," in Ralph Linton, editor, *The Science of Man in the World Crisis*, New York: Columbia University Press, 1945, p. 91.

² *Ibid.*, p. 97.

³ For earlier examples, see Ruth Benedict, *Patterns of Culture*, Boston: Houghton-Mifflin, 1934; Abram Kardiner, *The Psychological Frontiers of Society*, New York: Columbia University Press, 1945; Margaret Mead, *Sex and Temperament in Three Primitive Societies*, New York: Morrow, 1935. For more recent explorations of this relationship, see Paul A. Hauck, "Ute Rorschach Performances and Some Notes on Field Problems and Methods," Salt Lake City: University of Utah, *Anthropological Papers*, No. 23, 1955; Robert J. Havighurst and Bernice L. Neugarten, *American Indian and White Children: A Sociopsychological Investigation*, Chicago: University of Chicago Press, 1955; Ronald L. Olson, "Channeling of

Character in Tlingit Society," in Douglas G. Haring, editor, *Personal Character and Cultural Milieu*, Syracuse: Syracuse University Press, 1956, pp. 675-687.

⁴ Edward Rose and William Felton, "Experimental Histories of Culture," *American Sociological Review*, 20 (August, 1955), pp. 382-392.

⁵ John Collier, Jr., "Photography in Anthropology: A Report on Two Experiments," *American Anthropologist*, 59 (October, 1957), pp. 843-859; James A. Davis, "Status Symbols and the Measurement of Status Perception," *Sociometry*, 19 (September, 1956), pp. 154-165; Margaret Mead,

PROCEDURE

The Situational Conception of Culture and Social Structure and Their Relationship. Culture is conceived here situationally—as occasions featuring configurations of objects and of actors playing distinctive types of roles, each configuration having its standardized meaning and significance for the normally socialized person. The role configurations are overtly and concretely manifest in social situations and both the actors involved and outside investigators see and interpret the situations in terms of these configurations. Social structure is also conceived situationally—as pluralities in social situations featuring specific numbers of actors distributed in observable patterns within the space of the situation.

A close connection between culture and social structure, as defined above, may be anticipated. Since the role configurations have culturally standardized meaning and significance, most situation actors will interpret them in the same way as they do funerals, weddings, political rallies, parties, and the like. They will then attempt to adapt their own role play to the supra-personal requirements of the culturally defined occasion, and this will include certain spatial and contact requirements for playing the appropriate role successfully. For example, an obvious connection exists between such culturally standardized occasions as tennis matches and the structuring of the inter-actors into two spatially segregated sub-units of players and, sometimes, a third sub-unit of spectators.

Classification of Culture. A typology of cultural occasions was developed for this study by analysis of the role configurations displayed in approximately 6,000 photographs of a wide variety of American social situations.⁶ The hundreds of superficially differing role constellations encountered in these photographs proved to be classifiable in 26 categories, each of which involves a

distinctive type of role constellation and implies a distinctive type of situational process. For example, a considerable number of the photographs depict persons examining other persons and objects, and persons experimenting, exploring, inspecting, interviewing, surveying, testing, and so on. These may be grouped as exploratory occasions because each involves the general role of investigator and implies the basic action process that Parsons has termed "cognizing."⁷

At a higher level of abstraction, it was possible to reduce the 26 categories into five general types of occasions according to the general orientation of the central roles involved. This procedure yields: (1) "mental" type occasions, featuring idea- or thought-oriented central roles; (2) "utilitarian" type occasions, with central roles primarily oriented to utilitarian-pragmatic processes directly and immediately concerned with the acquisition, processing, distribution, exchange, or utilization of resources; (3) "social organization" type occasions, in which the central roles are primarily focused on the organization or control of situation personnel; (4) "social disorganization" type occasions, in which the central roles are primarily oriented towards disruptive and dissociative factors; and (5) "amusement" type occasions, in which the central roles are primarily focused on amusement, entertainment, relaxation, or play. The typology of 26 occasions grouped into the five general types appears in Table 2.

Classification of Social Structures. Two features of photographed social situations provide reference points for structural analysis: the size of a plurality in a situation and the distribution of its members in the situation space. Earlier studies by Simmel⁸ and more recent ones by Bales and his associates⁹

⁷ Talcott Parsons and Edward A. Shils, editors, *Toward a General Theory of Action*, Cambridge: Harvard University Press, 1951, pp. 68 ff.

⁸ See *The Sociology of Georg Simmel*, translated by Kurt Wolff, Glencoe, Ill.: Free Press, 1950, Part II, "The Isolated Individual and the Dyad," "The Triad," and "The Importance of Specific Numbers for Relations Among Groups," pp. 118-177.

⁹ Robert F. Bales and Edgar F. Borgatta, "Size of Group as a Factor in the Interaction Profile," in A. Paul Hare, Edgar F. Borgatta, and Robert F. Bales, *Small Groups: Studies in Social Interaction*, New York: Knopf, 1955, pp. 396-413.

"Some Uses of Still Photography in Culture and Personality," in Haring, editor, *op. cit.*, pp. 79-105; unsigned article, "Anthropologists Use Movies," *Clearing House Bulletin*, Society for Applied Anthropology, 3, No. 2 (1955), pp. 35-37.

⁶ A stratified random sample of photographs covering two pre-World War II and two World War II years from *Life* magazine.

attest to the theoretical importance of group size as a reference point for classification of group structures, while the sociological importance of distribution patterns is indicated in such investigations of the relationship between the spatial distribution of pluralities and interaction as those by Bavelas, Gullahorn, and Steinzor.¹⁰

A typology for classification of pluralities by size was developed which takes into account both the pioneering work of Simmel and such recent studies as those of Bales and Borgatta. These investigations indicate that certain breaking points in the size continuum are sociologically crucial and should be taken into consideration in the classification of groups by size. In the present analysis situation pluralities are classified as dyads, triads, small groups, medium groups, large groups, and masses.¹¹

A scheme for classification of pluralities according to the ways in which their members are distributed in the situation space was also developed. Accordingly, pluralities are classified according to the number of distinct sub-units into which a situation population is fragmented. To do this empirically, a concept of spatial segregation was formulated utilizing two objective criteria of segregation, functional-structural and physical. Persons are considered to be segregated from each other in the photographs if, one, they occupy functionally or structurally discrete parts of the situation space or, two, if they are not physically adjacent to each other or in the same chain of adjacency. Thus, for example, persons in an orchestra occupying a pit, performers on a stage, spectators seated in a box, and spectators seated in a section on the main floor of an auditorium would be considered segregated from each other irrespective of their actual physical proximity

because they occupy functionally or structurally discrete parts of the situation space or both. Two groups occupying seats in the same seating section of an auditorium but separated by sufficient physical distance to prevent their physical contact would be considered segregated because they are not adjacent.

A classification of spatial patterns was adopted which distinguishes those groupings in which all actors are unsegregated from those structured into two, three, four through nine, and ten or more segregated sub-units. In Tables 4 and 5 these groupings are termed respectively mono-polar, bi-polar, tri-polar, multi-polar, and fragmented structures.

Reliability of the Typologies. A test of the reliability of classifying photographed cultural occasions and group structures utilizing the criteria discussed above was made. Ten student judges in an undergraduate course on social research methods were employed in the test. The method was described for the students; they were then shown several properly classified photographs and the bases for the classification were explained. With this minimum training, the students were divided into two teams of five persons each. Each team member was presented copies of all of the typologies developed for classification, and asked to analyze and to classify a set of 40 photographs, working independently of the others. Two different sets of photographs were used, one for each team. These were selected by chance from four issues of *Life*, which had been previously analyzed, making possible a comparison of the classifications of eighty photographs by six independent judges. Table 1, which summarizes the results of this test of validity, indicates the high degree of reliability of the method.

The Data and the Sample. Every week hundreds of photographed pluralities are presented in such magazines as *Life* and *Look*. But such data, to the writer's knowledge, have never been systematically exploited specifically to describe the patterns of repeated co-actions and repeated relationships between co-actors, which Nadel has termed respectively the cultural and social dimensions of the human world.¹²

¹⁰ See David B. Hertz and S. L. Lesser, "People in Groups," *Scientific American*, 184 (February, 1951), pp. 26-28, for a summary of Bavelas' work; J. T. Gullahorn, "Distance and Friendship as Factors in the Gross Interaction Matrix," *Sociometry*, 15 (February-May, 1952), pp. 123-134; Bernard Steinzor, "The Spatial Factor in Face to Face Discussion Groups," in Hare, Borgatta, and Bales, *op. cit.*, pp. 348-353.

¹¹ Small groups are taken to consist of four through nine persons, medium groups ten through 19 persons, large groups 20 through 99 persons, and masses 100 or more persons.

¹² S. F. Nadel, *The Foundations of Social Anthropology*, Glencoe, Ill.: Free Press, 1953, pp. 75 ff.

TABLE 1. ANALYSIS OF 80 PHOTOGRAPHS: EXTENT OF AGREEMENT AMONG SIX INDEPENDENT JUDGES

Degree of Agreement	Type of Analysis					
	Occasion		Group Size		Spatial Patterns	
	Number	Per Cent	Number	Per Cent	Number	Per Cent
6 of 6	26	32.5	63	78.8	28	35.0
5 of 6	21	26.3	15	18.8	15	18.8
4 of 6	12	15.0	2	2.5	20	25.0
3 of 6	16	20.0	—	—	17	21.3
Less than 3	5	6.3	—	—	—	—
Total	80	100.0	80	100.0	80	100.0

These data have their limitations. They cannot, of course, be assumed to comprise a sample of all social situations as they actually occur in the American society since editorial policy and the rules of photographic style followed by news photographers may have influenced both the selection of the types of occasions and the points of focus within them. In addition, such technical limitations of still photography as scope and depth of field may have distorted the portrayal of situations. However, the data do depict Americans from a broad range of occupational and socio-economic statuses, in all regions of the country, in a wide variety of occasions, ecological settings, groupings, and forms of interaction. And they do exist in sufficient numbers for sound quantitative study. Within limits, they contain sociological information which may disclose otherwise inaccessible patterns of cultural and social relationships. Although they may not be validly used to generalize about either the relative frequencies with which occasions or sizes and structures of pluralities actually occur in American society, they may be employed for the more limited purposes of investigating relative structural differences in those occasions which are depicted.

The sample upon which the present investigation is based is a randomly selected issue of *Life* per month for 1944 and 1945, midyears of the 1940 decade. These issues contain 1,017 utilizable photographs of social situations. Photographs of single individuals, photographs used in advertisements, photographs of persons posed with no role play or identifiable context involved, and photographs involving no Americans were excluded. Only one photograph was taken

from a series depicting the same occasion and the same grouping so as to prevent over-representation. The method of stratified random sampling was adopted to assure representation of all phases of each year of the period studied since considerable social activity is known to follow an annual cycle, with differing types of occasions associated with each phase.

RESULTS

Culture and Size of Group. The data on the distribution of sizes of pluralities in the various cultural occasions are presented in Table 2; the occasions are grouped into the five general types. A chi-square analysis of these data results in the rejection of the null hypothesis; there is a significant relationship between type of cultural occasion and size of situation plurality.

Although a statistically significant association has been demonstrated, this fact does not tell the complete story of the relationship. Examination of the table shows that the distribution of the size patterns fluctuates considerably within the five general type categories. This indicates that the degree of association between cultural occasion and size of situation plurality might have been significantly higher had there been a sufficient number of instances in all cells of Table 2 to warrant a statistical analysis utilizing the full typology of 26 occasions.

For purposes of further analysis of the relationship between cultural occasion and group size, a simplified size distribution tabulation of pluralities of under ten and ten and over was made for the occasions (of the 26) with a frequency of 50 or more instances each in the sample. Analysis once

TABLE 2. DISTRIBUTION OF VARIOUS SIZES OF GROUPS IN 1,017 CULTURAL OCCASIONS

Occasions	Types of Groups						Total
	Dyads	Triads	Small	Medium	Large	Mass	
I Mental type	55	21	81	26	27	15	225
Exploratory	25	4	33	8	3	4	77
Planning and decision-making	16	10	24	8	11	6	75
Orientation, training, and counselling	14	7	20	7	9	4	61
Assessment	0	0	2	1	2	1	6
Spiritual	0	0	2	2	2	0	6
II Utilitarian type	78	30	88	28	21	7	252
Promotional	6	4	18	4	6	4	42
Deployment	6	5	20	7	7	1	46
Production and maintenance	36	9	34	11	4	0	94
Exchange	6	2	3	0	0	0	11
Consumption and personal maintenance	24	10	13	6	4	2	59
III Organizational type	28	17	37	10	8	8	108
Conjunctive	4	0	6	1	1	1	13
Rehabilitative	11	4	9	2	2	0	28
Control	6	4	6	0	1	0	17
Recognition and commemorative	7	9	16	7	4	7	50
Disciplinary	0	0	0	0	0	0	0
IV Disorganizational type	5	5	18	16	10	3	57
Disruptions and disasters	0	1	1	1	0	0	3
Crimes	0	0	0	1	1	0	2
Wars	2	2	11	4	3	0	22
Capitulations	0	0	2	5	1	1	9
Disjunctive	3	2	4	5	5	2	21
V Amusement type	114	46	127	35	40	13	375
Contests	2	3	4	3	4	3	19
Exhibitions	8	2	4	0	5	0	19
Spectacles and production	19	5	33	9	9	7	82
Recreational	27	9	26	9	9	3	83
Sociability	58	27	60	14	13	0	172
Total	280	119	351	115	106	46	1017

$\chi^2 = 42.00$; d.f. = 20; $P < .01$.

more revealed a statistically significant association. These results, presented in Table 3, indicate that cultural occasions, as more specifically designated in the typology of 26 categories, and group size are also significantly associated. It is apparent that smaller groupings occur with significantly greater frequency in sociability, production-maintenance, exploratory, and consumption-personal maintenance occasions than they do in spectacles and productions, orientation-training and counselling occasions, planning occasions, and recognition and commemorative occasions.

There are some surprises presented by this analysis of size distribution, as well as

results which might have been anticipated. The following observations, concerned only with those occasions represented by approximately 50 or more instances each in the sample, comprise a summary of the more significant findings.

1. The unusually high proportion of dyads and the unusually low proportions of large pluralities and masses in the production-maintenance occasions are perhaps surprising. The economic sector of the culture is generally considered to be the most highly organized, tending towards mass production achieved through intricate coordination of relatively large numbers of workers as, for example, on the factory assembly line. Yet

TABLE 3. DISTRIBUTION OF GROUPS UNDER TEN AND TEN AND OVER IN SELECTED OCCASIONS

Occasion	Under 10	10 and Over	Total
Sociability	145	27	172
Production and maintenance	79	15	94
Exploratory	62	15	77
Consumption and personal maintenance	47	12	59
Recreational	62	21	83
Spectacles and productions	57	25	82
Orientation, training, and counselling	41	20	61
Planning	50	25	75
Recognition and commemorative	32	18	50
Total	575	178	753

$$\chi^2=23.39; \text{d.f.}=8; P<.01.$$

the sample indicates a significant bias against larger pluralities in this type of occasion.

2. The very high proportions of triads in the recognition-commemorative and sociability occasions are also surprising. With respect to the former, one might anticipate that unusually high concentrations would be at the level of the larger pluralities since the success of the recognition-commemorative process would seem to depend on the occasion being public. With respect to the latter, if Simmel was correct in regarding triads as pluralities with the most divisive tendencies, one of the most surprising of all results in this study is the strikingly high proportion of triadic sociability occasions. The very purpose of sociability occasions is friendly affable fraternization. This would be destroyed by the divisive tendency for two to pair off against one.

3. The high proportion of planning and decision-making occasions which are large pluralities may or may not be considered surprising. One hypothesis receiving considerable support today is that decision-making has become increasingly concentrated among a few power figures who conduct such affairs in private.¹³ A competing line of argument, however, is advanced to the effect that American decision-making is less and less an individualistic process, and more and more falls to committees and peer groups, with individuals fearing to act without the sup-

port and sanction of such groups.¹⁴ The results of this study support the latter line of argument. The planning-decision-making process was undertaken in medium, large, and mass pluralities in about 33 per cent of this type of occasion, while only about 26 per cent of the total sample was made up of such pluralities.

4. Not surprising are the very high proportions of consumption-personal maintenance occasions that are dyadic and triadic and the low proportions that are large pluralities and masses. That this type of cultural occasion features the highest proportions of dyads and triads of all occasions is to be expected since they would seem to involve essentially routine activities oriented to satisfaction of non-social needs of ego, which do not require the presence of publics for ego's gratification.

5. Also not surprising are the unusually low proportions of deployment and recognition-commemorative occasions that are dyadic, the unusually high proportions of deployment occasions that are medium and large pluralities, and the unusually high proportions of recognition-commemorative occasions that are medium pluralities and masses. Deployment in a culture featuring highly organized and concentrated networks of mass transportation would typically involve the individual in larger pluralities. And, as noted above, the recognition-commemorative process would seem to require the presence of alters to be successful and satisfying.

6. The very low concentration of triadic pluralities in spectacles and productions is not surprising. One would expect these to be public type occasions involving larger pluralities.

7. One would expect as well the low proportion of sociability occasions that falls within the medium, large, and mass categories (15.70 per cent). The sociability process of friendly fraternization would seem best adapted to smaller and more intimate pluralities. This proportion is less than half (32.58 per cent) the proportion of

¹⁴ This is the basic argument presented by William H. Whyte, Jr., in *The Organization Man*, New York: Simon and Schuster, 1956. It is also the conclusion implicit in Riesman's thesis that an "other-directed" man is emerging in America who is unwilling to make decisions independently of the peer group; see David Riesman, *The Lonely Crowd*, New Haven: Yale University Press, 1950.

¹³ See, e.g., C. Wright Mills, *The Power Elite*, New York: Oxford University Press, 1956.

TABLE 4. DISTRIBUTION OF VARIOUS GROUP STRUCTURES IN 1,017 CULTURAL OCCASIONS

Occasions	Mono-polar	Bi-polar	Tri-polar	Multi-polar	Fragmentized	Total
I Mental type	88	65	33	29	10	225
Exploratory	34	19	12	11	1	77
Planning and decision-making	36	21	7	5	6	75
Orientation, training, and counselling	17	20	11	12	1	61
Assessment	1	1	2	0	2	6
Spiritual	0	4	1	1	0	6
II Utilitarian Type	82	85	30	46	9	252
Promotional	13	15	5	7	2	42
Deployment	14	11	6	10	5	46
Production and maintenance	26	36	13	18	1	94
Exchange	4	4	2	1	0	11
Consumption and personal maintenance	25	19	4	10	1	59
III Organizational type	52	34	11	7	4	108
Conjunctive	10	1	1	0	1	13
Rehabilitative	10	14	2	2	0	28
Control	7	6	4	0	0	17
Recognition and commemorative	25	13	4	5	3	50
Disciplinary	0	0	0	0	0	0
IV Disorganizational type	13	10	12	19	3	57
Disruptions and disasters	1	2	0	0	0	3
Crimes	0	2	0	0	0	2
Wars	4	3	3	11	1	22
Capitulations	0	1	5	3	0	9
Disjunctive	8	2	4	5	2	21
V Amusement type	195	88	31	48	13	375
Contests	6	3	3	5	2	19
Exhibitions	13	2	1	0	3	19
Spectacles and production	32	27	8	12	3	82
Recreational	40	17	5	18	3	83
Sociability	104	39	14	13	2	172
Total	430	282	117	149	39	1017

$$\chi^2=61.90; \text{d.f.}=16; P<.001.$$

such "public" occasions as recognition-commemorative occasions and spectacles and productions in these larger plurality categories.

Culture and Degree of Segregation in Pluralities. The frequency distributions of the various types of group structures in all of the 1,017 cultural occasions are presented in Table 4. A chi-square analysis was made of these distributions as a test of significant association between type of occasion and structures classified according to the degree to which the situation populations were segregated into distinct sub-units. As the findings shown in table 4 indicate, the

null hypothesis of no association may be conservatively rejected.

It seems significant that over two-thirds of the total chi-square value is attributable to just six of the twenty-five cell values. Three of the cells have frequencies substantially less than anticipated by chance: utilitarian type occasions that are mono-polar, the disorganizational type occasions that are mono-polar, and the organizational type that are multi-polar. Three others have frequencies which are substantially greater than anticipated: the mono-polar amusement occasions, the tri-polar disorganizational type, and the multi-polar disorganizational type. It seems

TABLE 5. DISTRIBUTION OF GROUP STRUCTURES IN SELECTED OCCASIONS

Occasion	Mono-polar	Bi-polar	Tri-polar	Multi-polar	Fragmentized	Total
Exploratory	34	19	12	11	1	77
Planning	36	21	7	5	6	75
Orientation, training, and counselling	17	20	11	12	1	61
Production and maintenance	26	36	13	18	1	94
Consumption and personal maintenance	25	19	4	10	1	59
Recognition and commemorative	25	13	4	5	3	50
Spectacles and productions	32	27	8	12	3	82
Recreational	40	17	5	18	3	83
Sociability	104	39	14	13	2	172
Total	339	211	78	104	21	753

$$\chi^2=69.34; \text{d.f.}=32; P<.001.$$

clear there is a bias against structural unity in utilitarian and disorganizational type occasions and a bias favoring such unity in organizational and amusement type occasions.

The data were examined further to establish the extent to which certain occasions within the five general types are associated with structural segregation. In Table 5, the distributions of various types of structures are tabulated for those of the 26 occasions represented in the sample by 50 or more instances each. The selected occasions and the structures were found to be significantly associated.¹⁵

Comparison of the expected frequencies computed for the chi-square analysis with the actual frequencies of Table 5 reveals the following significant facts. Three of the cell values are substantially below the expected frequencies: the orientation-training and counselling occasions of the mono-polar type, the production and maintenance occasions of the mono-polar type, and the sociability occasions of the multi-polar type. On the other hand, four of the cell values are substantially above the expected frequencies: the mono-polar sociability occasions, the bi-polar production and maintenance occasions, the tri-polar orientation-training and counselling

occasions, and the multi-polar recreational occasions. Clearly, biases show up against unsegregated structures occurring in orientation-training and counselling, and production-maintenance occasions, and in favor of unsegregated structures in sociability occasions.

SUMMARY

1. Photographic data, which provide a large and diversified sample of pluralities in a variety of situations, may be utilized systematically to gain new insights into the relationship between culture and social structure. As manifest in photographed social situations, both culture and social structure may be reliably classified utilizing the typologies developed for this study and quantitative analyses may be made of their relationship.

2. Statistical analyses demonstrate a significant association between the kind of cultural occasion for which a plurality is gathered and its structure. Both the sizes of the pluralities and the degree to which their members were segregated in distinct sub-units in the situation space are associated with the type of cultural occasion at conservative levels of significance.

3. The present study sheds some light on the problem of identifying those cultural occasions which tend to feature smaller pluralities as contrasted to those tending towards larger groupings. A bias towards smaller groupings appears in connection with sociability, production-maintenance, exploratory, and consumption-personal maintenance oc-

¹⁵ The estimate of P (see Table 5) is based on Snedecor's suggestion that t (a standard measure in terms of sigma) be calculated, and the probability of exceeding the value be noted utilizing a cumulative normal frequency distribution table. In this case $t = 3.84$, and the probability of exceeding this value is less than .001. See George W. Snedecor, *Statistical Methods*, Ames, Iowa: Iowa State College Press, 1946, p. 194.

casions; and a bias towards larger groupings in connection with spectacles and productions, orientation-training and counselling occasions, planning occasions, and recognition-commemorative occasions.

4. The study also helps to clarify the identification of those cultural occasions in which pluralities tend to break up into a number of distinct sub-units, as contrasted with those in which they tend to remain

spatially unified. A bias against structural unity appears in utilitarian and disorganizational type occasions, with a contrary bias in organizational and amusement type occasions. In addition, there are biases against unsegregated structures occurring in orientation-training and counselling occasions and in production-maintenance occasions, as well as biases in favor of such structures in sociability occasions.

RESEARCH REPORTS AND NOTES

THE COMMUNITY SOCIAL PROFILE

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Sociologists, who for a long time have been subjecting the community to intensive study, are frequently asked by action-oriented professional persons to assist in the preparation of the studies they undertake or to review the results they have obtained. Such assistance, which includes the careful analysis of all available statistical data, must go beyond these figures to community characteristics not already quantified. What is called for in many cases is a short-run, easily administered method which, though lacking in completeness of detail, nevertheless is accurate as far as it goes and at the same time penetrating enough to be helpful in program planning. This is a report of one such method which, through a clearly-formulated reconnaissance, makes possible the preparation of a community social profile essentially sociological in content. Its utility has been tested over a twenty-year period in states as widely diverse as New York, Alabama, Kentucky, and Massachusetts.

THE FEATURES OF THE METHOD

The research team. In any short-cut approach there must be relatively little waste motion. Thus research assistants must already have some skill in interviewing, some knowledge of how the data are to be processed, and some understanding of the community as a sociological concept. The tasks confronting such a research team are the interviewing of from 25 to 40 people in a local community, pulling together pertinent statistical support, and collecting available maps, documents, and newspapers which have some bearing upon the community situation. It is important to stress that this is essentially a sociological and not a social action undertaking. Its purpose is to describe competently and economically the chief social traits of the community; it is not designed to train the untrained citizen in community survey techniques.

Where possible, four or five people are formed into a team in preference to the more extended labors of a lesser number. This builds more cross-checking into the operation because more trained people are reacting to the community

and interacting with each other. Ordinarily, each interviewer averages from two to three interviews per day, that is, ten to fifteen man-days in the cases of communities where 30 interviews suffice. Four or five workers can carry out the necessary data collection in less than a week's time.

Data collection. As already indicated, the chief schedule is one designed for administration to community leaders who, as experience has shown, have more knowledge about the community since they are accustomed to think of problems in a broader context than those whose major interests center almost exclusively around their jobs and their own homesteads. Experience also shows that, far from presenting a united and closed front, these leaders reveal in their responses the existence of the major groupings and social divisions one must investigate.

The "Community Leader Schedule" has undergone some modifications through the years but it remains essentially open-ended. It calls for data about the informant, social organizations and their leaders, and the "institutional" areas of church, school, economy, recreation, and local government. It asks what the informant likes best about the community and what he considers to be its chief problems, about social issues over the past five years, and major changes underway; and it asks the informant to list eight people whom he considers to be important community leaders and to give the reasons for their selection. Many of these questions, such as the one requesting the listing of community leaders, are carefully phrased and presented to each informant in a standardized way. But usually the main purpose of the questions is to prompt the informant to talk about his community with respect to certain topics set forth on the schedule. When these comments are fully recorded it turns out that what some people have called a "deceptively simple" schedule has elicited a wealth of valuable information.

But who are these key leaders to be interviewed? How are they determined? Before beginning a community study an effort is made to identify some of them, with whom appointments are arranged by telephone. When this is not possible the first three or four schedules are "wasted," that is, people are contacted who occupy positions which would seem to give them some knowledge of the community and who, in

addition to answering other questions, are asked to name the eight most important leaders.¹ After two or three interviews the tabulation of the names mentioned may be initiated and subsequently interviews held with the "most frequently mentioned." Communities vary, of course, in the extent to which the leadership is concentrated and the frequency with which the same people are mentioned. Nevertheless the leadership structure begins to emerge quite clearly after only a few interviews with well-informed citizens. In some studies, even of cities of 100,000 inhabitants, twenty interviews cover all of the leaders mentioned as many as three times, illustrated by the actual distribution for such a city shown in Table 1.

TABLE 1. FREQUENCY DISTRIBUTION OF MENTIONS AS KEY COMMUNITY LEADERS, NEW BEDFORD, MASSACHUSETTS, 1958

No. Times Listed (Out of 23 Possibilities)	No. Persons Listed This No. of Times	Total Mentions (Column 1 \times Column 2)
18	1	18
16	1	16
15	1	15
12	1	12
11	1	11
10	1	10
8	2	16
5	6	30
4	2	8
3	4	12
2	8	16
1	34	34
62 people		198 mentions

It is assumed that completing the total universe of those named as leaders, which is the crucial aspect of this method, would not have changed the rank order materially, nor would it have brought to light more than five or six other individuals not previously named. Long before all of those listed have been interviewed one is able to predict many of the answers an informant will give, but the interviewing is continued in the hope that exceptions to the rule will be uncovered and thus reveal facets of community life that might not otherwise emerge. The team members find little difficulty in deciding where to "cut-off" the interviewing for

they agree that they have gone well beyond the point of diminishing returns.

The leaders, however, do not necessarily represent all of the important social divisions which have been identified in the community. In this case, interviews are conducted with the spokesmen for these other groupings, starting with those suggested by the community leaders when they were asked to describe the community's significant social divisions. The interviews with these subleaders seek to determine how the divisions they represent fit into the overall community framework and only incidentally delve into the psychological problems of those involved.

Another aspect of the collection of data deals with contradictions in perceptions on the part of the informants. It is assumed from the beginning that these differential perceptions will be encountered, but it is expected that in some measure they will be consistent with socio-economic characteristics and other social traits uncovered. When these perceptions run counter to previous experience or to theoretical expectations further interviewing is required to account for the variation. In such cases, some leader already interviewed who has demonstrated a capacity to analyze such matters objectively may be revisited and asked to clear up the seeming difficulty.

The interview materials are checked with data collected from other sources. These include local histories, promotional literature prepared by the Chamber of Commerce, surveys and studies in such applied fields as health and education, statistical series of various kinds, and local newspapers. All of this material not only serves to cross-check what informants say but provides a depth of detail which proves most useful in preparing the community profile.

Analysis and preparation of the social profile.

As soon as the field work has been completed the research team meets to decide on those social traits² which require stress if a useful picture of the community is to be obtained. This selection is necessary on two counts: description of all traits would not be in order even if there were data available since the purpose of the social profile is to highlight rather than to catalog the characteristics of the community; there are some traits not central to the inquiry

¹ This approach may be compared with the approaches followed by Floyd Hunter, Delbert C. Miller, Peter H. Rossi, Robert O. Schulze, and others who have studied "power structure," "influentials," "economic dominants," and other types of local elites.

² These traits include those that sociologists would conventionally look into: social divisions and other evidences of stratification; institutional behavior (religion, local government, economy, education, family, recreation); major organizations and their interaction; evidences of major current changes; leadership structure; issues or major community problems as defined locally; indications of competing or conflicting value systems.

about which the team believes that insufficient data have been collected for an adequate treatment. Both the research team and the reader need to keep in mind what can be stated with confidence and what should be set forth as mere conjecture.

By using practically the same schedule in every community almost the same ground is covered in each study, permitting some comparability between communities. This very comparability allows the selection of a special combination of traits for a particular community as the pattern best describing its social features. This is why Tables of Contents of social profiles will read differently. Yet these variations grow out of what is essentially a comparative approach.

The traits to be described form the working outline in terms of which the analysis of the data proceeds. The data relating to each trait are excerpted on cards so that the person writing up that section will have before him all pertinent comments. Different team members are assigned different topics in keeping with their training and with the types of informants they had encountered as the interviewing appointments were made.

Needless to say, any differences in points of view among the team members about a particular trait are ironed out by re-examination of the data, by a telephone call to some competent local informant, or by another field trip to the community. In preparing the study budget provision is always made for a final "clean-up trip" when the report itself is nearing completion. But such a trip is seldom necessary if the methods outlined above have been carefully followed.

UTILITY AND APPLICATION OF THE APPROACH

This approach, of course, has a serious limitation: it is idiosyncratic, being designed to gather information about one community to serve some particular purpose. Those who engage in several successive studies of this sort gain a general understanding of community structure and processes although the methods used are not designed to demonstrate its validity. Therefore this approach does not have the cumulative value that other types of research may possess.³

³ Samuel A. Stouffer is directing a project, in which the present writer is also engaged, designed to break through the idiosyncratic limitation of this approach. Through the use of special schedules and pre-determined types of informants, an effort is being made to standardize information gained about cleavages, issues, and key leadership structure so that many communities can be studied simultane-

Granting this limitation, it in no way reduces the utility of a well-executed reconnaissance report for those professionally interested in local social action programs.

In New England recently some of its possible applications have proven very interesting. An industrial firm wanted to know which of two New England towns was a better location for a new industrial facility on the basis of social traits alone. Another concern was interested in the connection between the controversy over industrial zoning and the general community traits; a third wished to learn whether or not a given community was a place to which engineers would be attracted.⁴ In each of these cases the point was accepted that the easiest way to answer the central question was to use the reconnaissance method described here. Only after the overall social profile had been completed could a special "peel-off" report be prepared concerning the specific questions which had initiated the larger study. This same approach is adapted for use by local health departments, planning boards, school boards, library boards, and United Community Services, the representatives of which see their groups' activities in a new perspective when confronted with the overall picture provided by the community social profile.

RURAL-URBAN FERTILITY DIFFERENTIALS IN MEXICO*

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Rural-urban fertility differentials in Mexico have been the subject of several investigations. Whetten,¹ Moore,² Davis,³ and, more recently,

ously and generalizations can be derived. Field tests of the instruments are still underway.

*These queries and the resulting studies have been channeled through the Community Analysis Division of Associates for International Research, Inc., a private applied social science research company located in Cambridge, Massachusetts, of which the writer has been serving as part-time Research Director.

*A. J. Coale and F. W. Notestein, both of Princeton University, kindly offered critical comments on an earlier draft of this paper.

¹ Nathan L. Whetten, *Rural Mexico*, Chicago: Chicago University Press, 1948, pp. 389-393.

² Wilbert E. Moore, *Industrialization and Labor*, Institute of World Affairs and New School for

Burnight, Whetten, and Waxman⁴ have treated this question, reaching substantially the same conclusion. As the last-named authors put it: "The most obvious conclusion coming out of this study is that Mexican fertility is subject to the differential effect of urbanization in much the same way as has been fertility in the industrialized countries of the West."⁵

This paper reexamines Mexican rural-urban fertility differentials and, particularly, the apparent trends in these differentials over time. The data come from the series of decennial censuses taken in Mexico in 1930, 1940, and 1950, and the measures of fertility used include both the fertility and marital fertility ratios.

While the official Mexican Census definition of "urban" refers to those places with populations of 2,500 and over, investigators most familiar with Mexican urbanism suggest that 10,000 would be a more realistic lower limit.⁶ At any rate, the most clearly urban element of the Mexican population presumably should be found in the largest cities, those of 50,000 and over. A comparison of these cities with the rural elements of the population should bring out clearly such fertility differentials that exist.

METHOD

No problem was encountered in finding the age distributions for cities of 50,000 and over in 1930 and 1940, as this information was published separately. The year 1950 did pose a special problem, however, since age distributions were published for only Mexico City and Madero City (both in Distrito Federal). It was found that the figures published for municipios (the smallest geographical division above localidad and roughly comparable to county in the United States) in 1950 could be taken as good estimates of the cities contained in these municipios.

Social Research, Ithaca: Cornell University Press, 1951, p. 221.

³ Kingsley Davis, "Population Trends and Policies in Latin America," *Some Economic Aspects of Post-war Inter-American Relations*, Latin American Studies II, Austin: University of Texas, 1946, pp. 35-39; also, Kingsley Davis and Anna Cassis, "Urbanization in Latin America," *Milbank Memorial Fund Quarterly*, 24 (April, 1946), pp. 186-207, and 24 (June, 1946), pp. 292-314.

⁴ Robert G. Burnight, Nathan L. Whetten, and Bruce D. Waxman, "Differential Rural-Urban Fertility in Mexico," *American Sociological Review*, 21 (February, 1956), pp. 3-8.

⁵ *Ibid.*, p. 8.

⁶ Whetten, *op. cit.*, p. 36; and Floyd Dotson and Lillian Ota Dotson, "Urban Centralization in Mexico," *Rural Sociology*, 21 (March, 1956), p. 42.

The population of the municipio was compared to that of the city it contained and, when the city comprised more than 70 per cent of the municipio, the age distribution of the municipio was used as that of the city. The resulting error introduced into the final figures—that is, the amount of rural population included in the totals of cities 50,000 and over—must be small since the population of the 18 cities (whose populations were estimated in this manner) amounts to 90.2 per cent of the population of the 18 municipios involved. The cities very nearly filled up the municipios and, moreover, some of the unaccounted ten per cent can be assumed to be suburban.

A second problem posed by the 1950 Census figures was that of deriving the number of married females aged 14-to-39. The data available were the numbers of married females aged 14-and-over, females aged 14-and-over, females aged 14-to-39, and females aged 40-and-over. Thus, the figures shown in Tables 1 and 2 for married females aged 14-to-39 in 1950 are based on the following assumptions: first, that the number of married persons in the 14-to-39 age category plus the number in the 40-and-over age category equals the total number of married females listed in the 1950 Census; and, second, that the per cent married in each of these two age categories is the same as the per cent married in the 14-and-over age category.

By 1940, for all Mexico and urban Mexico (10,000 and over), the differences in the per cent married 14-to-39 and 40-and-over were small, and the trend seemed to indicate a convergence. The per cent married 14-to-39 was falling (presumably due to rising age at marriage), while the per cent married 40-and-over was rising (presumably due to the lowering of mortality and the consequent reduction of widowhood). Thus, the assumptions made by the estimation process do not seem unreasonable.

Even if a small error is contained in Table 1, the impact of minor changes in the per cent married by age group on the child-to-woman ratio would be negligible. Its effect would be that all the ratios computed using these estimated married females 14-to-39 would be higher than they should be (that is, higher than their true trend value compared to earlier years). But the comparison between rural and urban for 1950 should not be affected by such error.

THE MEASURE USED

Some criticisms are made of the use of the fertility ratio. Burnight, Whetten, and Waxman note the three main reasons why this ratio may not, in any given situation, be a very accurate estimator of the actual number of births occur-

TABLE 1. FERTILITY RATIOS FOR MEXICAN CITIES OF 50,000 AND OVER AND REMAINDER OF THEIR STATES, 1930-1950¹

Localities	Children 0-4 per 1,000	Indexes	Children 0-4 per 1,000 Married	Indexes
	Females 15-49		Females 14-39	
Cities of 50,000 and over				
1930	376	62	983	83
1940	386	63	915	76
1950	493	74	1155	90
Rural ²				
1930	607	100	1193	100
1940	616	100	1196	100
1950	669	100	1290	100

¹ Sources:

1950

1. States total 0-4 and all females 15-49: *Septimo Censo de Poblacion, Resumen General, Cuadros 4 and 2-A.*
2. Total population of both the cities and their municipalities: *Integracion Territorial de los Estados Unidos Mexicanos, Septimo Censo General de Poblacion, S. de E., D. G. de E., 1952.*
3. Age distribution for municipios: Individual state volumes, Cuadro 2.

1940

1. States total 0-4 and all females 15-49: *6° Censo de Poblacion, 1940, Resumen General.*
2. Married females 14-39: *Ibid.*
3. All figures for cities: Comparable tables of individual state volumes.

1930

1. States total 0-4 and all females 15-49: *6° Censo de Poblacion, Resumen General.*
2. Married females 14-39: Cuadro XIV of individual state volumes, *5° Censo de Poblacion.*
3. Figures for cities: *5° Censo de Poblacion, Resumen General, Cuadros LX and LXII.*

² States minus cities of 50,000 and over.

ring in two populations: (a) differences in rural-urban infant and childhood mortality, (b) rural-urban differences in the accuracy of the census enumeration of children, and (c) differences in the age structure of the female population in the fertile ages. These authors analyzed the last of these factors and found a statistically significant correlation between the proportion of females aged 15 to 19 in the 15-to-49 age group and the fertility ratio of the state. In other words, those states with the highest fertility ratios also had the highest concentration of their potentially fertile female population in the very young (15 to 19) age category. Since these younger ages are generally assumed to be the most fertile, these authors conclude that, along with urbanization, variations in the age composition of the female population help to explain the observed variations in the fertility ratios of the states of Mexico in 1950. They attribute these age composition differentials to internal migration. One could argue with equal plausibility, however, that the differences in the age composition of the state populations of Mexico are themselves due to differences in fertility. If the relative levels of fertility have increased during the past few decades, a greater percentage of the population is now in the younger

age brackets.⁷ Differences in the fertility ratios of two populations, however, could also arise because of the first two factors mentioned above—accuracy of census enumeration and infant mortality.

THE DATA

Table 1 shows the fertility ratios for all cities of 50,000 and over and for the "rural" population, that is, the total of those states containing cities of 50,000 and over minus the cities.⁸ It should be noted that cities of this size in Mexico have steadily increased in number: in 1921, ten cities were in this category, in 1930 eleven, in 1940 thirteen, and in 1950 there were 20 such cities.

⁷ On the effect of fertility on the age distribution, see A. J. Coale, "How the Age Distribution of a Human Population Is Determined," *Cold Spring Harbor Symposia on Quantitative Biology*, Vol. 22, 1957, pp. 83-89.

⁸ This approach has the disadvantage of including as "rural" any cities of less than 50,000 located in these states. But it has the advantage of comparing the large cities with contiguous rural areas, thus minimizing any differentials which might arise because of sectional differences in the population. This is a very real advantage in a country like Mexico where great economic, social, and ethnic differences exist among regions.

TABLE 2. MEXICAN FERTILITY RATIOS BY SIZE OF PLACE, 1930-1950¹

Locality and Year	Children 0-4 per 1,000 Females 15-49	Indexes	Children 0-4 per 1,000 Married Females 14-39	Indexes
Cities of 50,000 and over				
1930	376	61	983	82
1940	386	60	915	76
1950	493	74	1155	89
Cities of 10,000 to 50,000				
1930	402	65	1057	88
1940	436	68	1004	83
1950	569	85	1249	96
Rural Mexico ²				
1930	620	100	1206	100
1940	641	100	1206	100
1950	668	100	1300	100
Total Mexico				
1930	575	93	1175	97
1940	580	90	1150	95
1950	626	94	1272	98

¹ Sources:

Total Mexico—1950: *Septimo Censo de Poblacion*, Cuadro 2, for total persons 0-4 and females 15-49; married females 14-39 approximated as explained in text.

1940: For total persons 0-4 and females 15-49 and married females 14-39—6° *Censo General de Poblacion, Resumen General*.

1930: Total persons 0-4 and females 15-49—6° *Censo General de Poblacion*; married females 14-39—*Quinto Censo de Poblacion, Resumen General*, Cuadro XXII.

Cities—See sources to Table 1. The cities for 1950 were obtained indirectly as explained above. The sources cited for cities of 50,000 and over in 1940 and 1930 are also the sources for the cities of 10,000 to 50,000.

For technique used in estimations of married females see text. The figure for cities of 10,000 to 50,000 for 1950 represents 36 cities of a total of 135.

² Total Mexico minus cities of 10,000 and over.

Differentials in the urban and rural fertility ratios appear, but they have been changing over time. In 1930 the marital fertility ratio (ratio of children aged 0 to 4 to married females aged 14 to 39) for the cities was 83 per cent of the ratio for the rural areas; by 1950 it had risen to 90 per cent. The same narrowing differential is reflected in the fertility ratio (ratio of children aged 0 to 4 to all females aged 15 to 49), but in 1950 urban fertility was only 74 per cent of the rural.

Some analysis of the fertility of cities of 10,000 to 50,000 population is also possible. In Table 2 the cities of this size class are considered on a nationwide basis. In this case, "rural" refers to total Mexico minus cities of 10,000 and over (in other words, minus the cities being analyzed but also minus cities of 50,000 and over.) The ratios for cities of 50,000 and over and for the entire country are also presented for purposes of comparison.

The cities of 10,000 to 50,000 show much the same general trend as the largest cities, except

that the rural-urban differentials are consistently smaller. Once again, the marital fertility ratio shows a reduced rural-urban fertility differential: in 1950 the urban marital fertility ratio was 96 per cent of the ratio for rural Mexico.

Another piece of evidence on Mexican rural-urban fertility differentials is offered by a special sample drawn from the 1950 Census cards by the *Direccion General de Estadistica*.³ Table 3 summarizes the fertility ratios based on this sample.

³ The sample was drawn for the five major geographical regions of Mexico. At random, one locality with a population of 20,000 to 30,000 and five localities with populations of less than 2,500 were selected for each of the five regions. The sample totaled more than 150,000 persons and the coefficient of variation was ± 7.49 . The sample was drawn under the direction of Miss A. M. Flores, of the *Direccion General de Estadistica*, for a study of Mexican fertility trends conducted by the Office of Population Research, Princeton University. The Office of Population Research and the author are indebted to Miss Flores and her colleagues.

TABLE 3. MEXICAN FERTILITY RATIOS, SPECIAL RURAL-URBAN SAMPLE BY REGION, 1950¹

Localities	Children 0-4 per 1,000 Females	
	15-49	Indexes
<i>Region I</i>		
A) Locality of 20,000-30,000	642	97
B) Total: Five Random Rural Areas ²	664	100
<i>Region II</i>		
A) Locality of 20,000-30,000	638	93
B) Total: Five Random Rural Areas	685	100
<i>Region III</i>		
A) Locality of 20,000-30,000	563	82
B) Total: Five Random Rural Areas	684	100
<i>Region IV</i>		
A) Locality of 20,000-30,000	510	71
B) Total: Five Random Rural Areas	718	100
<i>Region V</i>		
A) Locality of 20,000-30,000	543	99
B) Total: Five Random Rural Areas	549	100
<i>Totals</i>		
A) Localities of 20,000-30,000	581	89
B) Random rural Areas	650	100

¹ Source: Direccion General de Estadistica; Letter of February 27, 1957, to A. J. Coale, Office of Population Research, from A. M. Flores, Direccion General de Estadistica.

² Rural = less than 2,500 population.

This sample is an especially useful supplement for the non-random sample for cities of 10,000 to 50,000 on which Table 2 is based. Both Tables 2 and 3 show fertility ratios for this size-class of urban localities which are just under 90 per cent of the fertility ratio for rural Mexico.

The sample figures underlying Table 3 indicate wide fluctuations in the ratios for both cities and rural localities among regions. In fact, the largest differentials are geographic ones. One of the rural localities in Region V is only about 40 per cent of a rural locality in Region II. Similarly, the urban center in Region IV is only about 80 per cent of the urban center in Region I. Even allowing for sampling error, large regional differences seem to be clearly present.

CONCLUSIONS

These data permit the following conclusions:

1. Differentials in fertility ratios between rural and urban Mexico existed between 1930 and 1950. Marital fertility ratios in this period show smaller differentials, however, emphasizing

the importance of the differential rural-urban incidence of marriage in Mexico for fertility.

2. Only modest differentials existed by 1950. The marital fertility ratios of the largest cities were some ten per cent below rural marital fertility ratios while cities of 10,000 to 50,000 had marital fertility ratios only four per cent below rural ratios.

3. Since about 1930 there seems to have been a narrowing of the differentials. This is true for both the fertility and marital fertility ratios and appears to have occurred because of rising urban fertility coupled with fairly stable rural fertility. The notion that rural-urban fertility differentials in Mexico are attributable to the effects of industrialization is open to question. Surely Mexico was less industrialized in 1930 than in 1950, yet urban fertility appears to have been higher in 1950 than in 1930.

4. All of these conclusions are tentative, since they are based on the fertility ratio, and this measure is subject to considerable error.

5. In particular, differences in rural-urban infant mortality represent a very likely explanation of some of the past differences in fertility ratios. Infant mortality in the cities of Mexico was probably much higher than in the rest of the country in 1930.¹⁰ If the percentage decline in infant mortality since 1930 has been greater in the cities than in the rural areas, the observed rise in urban fertility ratios could have occurred even without any change in real fertility.

6. In any case, it is by no means clear that the likely effect of further urbanization or industrialization on Mexican fertility will be a reduced birth rate.

A NOTE ON SOCIAL MOBILITY, RELIGIOSITY, AND STUDENTS' ATTITUDES TOWARDS PREMARITAL SEXUAL RELATIONS

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What is the relation of social mobility and religiosity to sexual attitudes and behavior? To what extent can differences in religiosity account for relationships found between social status and sexual behavior? In an attempt to clarify these questions, we review briefly some

¹⁰ This was the case for most Western populations well into the twentieth century and is the case in nearly all non-industrialized populations. See Arthur Newsholme, *Vital Statistics*, revised edition, London: Allen and Unwin, 1924, Chapter 24.

previously reported findings and discuss some pertinent original data.

According to Kinsey, socially mobile men (although not women) tend to adopt at an early age the sexual behavior of the social groups into which they will ultimately move.¹ Although Kinsey also finds differences in sexual behavior associated with variations in religiosity,² they are overshadowed by the much larger differences that are found among the various social levels.³

Dedman's finding of a relationship between higher religiosity and more restrictive attitudes towards premarital relationships among college men complements that of Kinsey on behavior. She reports little association, however, between sexual attitudes of the men and the social level of their parents.⁴

FINDINGS

Data obtained from a representative sample of 225 unmarried students at a co-educational liberal arts college are consistent with all the findings cited above, except the last. These data indicate that sex, social mobility, and religiosity are each related to attitudes toward premarital intercourse and to the incidence of heterosexual experience.

¹ Alfred C. Kinsey, Wardell B. Pomeroy, and Clyde E. Martin, *Sexual Behavior in the Human Male*, Philadelphia: Saunders, 1948, Chapter 11; also Kinsey et al., *Sexual Behavior in the Human Female*, Philadelphia: Saunders, 1953, pp. 182 and 338.

² Kinsey . . . , *Human Female*, op. cit., pp. 174 and 331.

³ Kinsey . . . , *Human Male*, op. cit., pp. 477 and 479.

⁴ Jean Dedman, "The Relationship Between Religious Attitude and Attitude Toward Premarital Sex Relations," *Marriage and Family Living*, 21 (May, 1959), pp. 171-176.

1. The women are more restrictive in both attitudes and behavior than the men: 43 per cent of the men but only 15 per cent of the women ever had sexual relations; 18 per cent of the men and 48 per cent of the women have restrictive attitudes towards premarital relations, while 58 per cent of the men and 31 per cent of the women have permissive attitudes; and 24 per cent of the men and 21 per cent of the women favor a double standard implying permissiveness for men but not women.

2. Students of higher religiosity are more restrictive than those of lower religiosity.⁵ In the former group, 29 per cent of the men and 64 per cent of the women are opposed to premarital relations; in the latter group, 12 per cent of the men and 42 per cent of the women are opposed (see Table 1). Among those of higher religiosity, 31 per cent of the men and seven per cent of the women ever had intercourse; the corresponding figures for men and women of lower religiosity are 48 per cent and 18 per cent, respectively (see Table 2).

3. Upwardly mobile students are more restrictive than stable students.⁶ In the mobile group, 28 per cent of the men and 65 per cent of the women have restrictive sexual attitudes; in the

⁵ Those who consider their religious group important were classified as higher in religiosity, and those who do not consider their religious group important or who do not belong to any religious group as lower.

⁶ Students who come from families in which neither parent is a college graduate were classified as upwardly mobile and those that come from families in which at least one parent is a college graduate as stable. It is assumed that some sociologically meaningful distinction, such as "upper middle" versus "lower middle" class exists between families of students on the basis of their parents' education, and that college students who come from non-college families constitute, on the whole, a group that is socially mobile.

TABLE 1. STUDENTS' ATTITUDES TOWARDS PREMARITAL RELATIONS, BY SEX, RELIGIOSITY, AND MOBILITY¹

Religiosity	Percentage Who Have Restrictive Attitudes ² Among					
	Mobile Men	Stable Men	All Men	Mobile Women	Stable Women	All Women
Higher	33	26	29	80	61	64
Lower	23	10	12	61	36	42
Higher and Lower Combined	28	14	18	65	43	48

¹ Percentages are based upon 108 men and 104 women. Excluded from this table are 13 respondents who did not answer questions about either religion or parents' education.

² The percentages of those holding permissive attitudes cannot be found by subtracting the percent with restrictive attitudes from 100 because the data are based upon a three-fold breakdown of respondents into those who are permissive, those restrictive, and those who favor a double standard involving freedom for men but not women.

TABLE 2. INCIDENCE OF SEXUAL RELATIONS AMONG STUDENTS, BY SEX, RELIGIOSITY, AND MOBILITY¹

Religiosity	Percentage Who Ever Had Relations Among					
	Mobile Men	Stable Men	All Men	Mobile Women	Stable Women	All Women
Higher	25	35	31	0	9	7
Lower	46	48	48	17	19	18
Higher and Lower Combined	36	45	43	13	16	15

¹ Percentages are based upon the same 108 men and 104 women as are those of Table 1.

stable group, 14 per cent of the men and 43 per cent of the women are restrictive (see Table 1). Thirty-six per cent of the men and 13 per cent of the mobile women, and 45 per cent of the men and 16 per cent of the women in the stable group ever had sexual relations (see Table 2).

4. When religiosity is held constant, the relationship between mobility and sexual attitudes persists. Among students of higher religiosity, 33 per cent of the mobile and 26 per cent of the stable men oppose premarital intercourse; for the mobile and stable women in this group, the figures are 30 per cent and 61 per cent, respectively. Among those of lower religiosity, 23 per cent of the mobile and 10 per cent of the stable men, and 61 per cent of the mobile and 36 per cent of the stable women are restrictive (see Table 1).

5. The relationship between mobility and sexual behavior persists among students of higher religiosity: 25 per cent of the mobile and 35 per cent of the stable men, and none of the mobile and nine per cent of the stable women in this group had experienced intercourse. Among those of lower religiosity, however, there is little difference in behavior between the two groups: 46 per cent of the mobile and 48 per cent of the stable men, and 17 per cent of the mobile and 19 per cent of the stable women ever had sexual relations (see Table 2). Thus it appears that mobility is related to the incidence of sexual relations among students of higher religiosity, but only to a slight extent among those of lower religiosity.

6. At least among the men, there is a definite relationship between religiosity and mobility: 48 per cent of the mobile men but only 28 per cent of the stable men said that religion is important to them. The corresponding figures for mobile and stable women are 22 per cent and 28 per cent, indicating, if anything, a slight negative relationship.

DISCUSSION

The relationship between religiosity and sexual attitudes and behavior might be accounted

for by the assumption that families that instill religiosity in their children at the same time inculcate in them restrictive sexual codes. Since the Judeo-Christian religious tradition dominant in our society condemns sexual activities outside of marriage, greater adherence to restrictive attitudes would be expected to be merely the obverse of greater internalization of the religious mores.

The relationship between social mobility and sexual attitudes and behavior may be interpreted as an instance of overconformity by the mobile persons to the restrictive norms of the stable group.⁷ Families that train their children for eventual mobility, it may be argued, through a process of anticipatory socialization,⁸ also teach them the mores, including the sexual codes, thought to be appropriate for "higher" social positions.

Can the relationships between social mobility and sexual attitudes and behavior be accounted for by differences in religiosity? Our data for the men⁹ are consistent with the hypothesis that the religiosity that may at an early age be instilled into some persons born into lower so-

⁷ It is assumed that the sexual norms of non-college families are generally more permissive than those of college families. This assumption is consistent with Kinsey's finding that the higher the social level of his single male subjects, the lower the incidence of intercourse. See Kinsey . . . , *Human Male*, op. cit., p. 550. This finding of Kinsey's, however, is difficult to reconcile with data that indicate that middle-class parents are more permissive in sexual socialization than lower-class parents. See Urie Bronfenbrenner, "Socialization and Social Class through Time and Space," in Eleanor E. Maccoby et al., editors, *Readings in Social Psychology*, New York: Holt, 1958, p. 413. Bronfenbrenner reports this difference between middle- and lower-class families only, which greatly limits or eliminates the possibility of comparison of his results with the data reported here.

⁸ Cf. Robert K. Merton, *Social Theory and Social Structure*, Glencoe, Ill.: Free Press, 1957, p. 265.

⁹ An adequate explanation of the slight negative relationship between religiosity and mobility among the women remains to be given.

cial levels tends both to facilitate their later social mobility and to shape their sexual attitudes. These data are also consistent with the more conservative hypothesis, which we prefer, that the same type of socialization process¹⁰

¹⁰ The specification of the nature of this socialization process, and of the factors that determine that at an early age some children begin to tread the road of social mobility and others do not, remains a problem for further research.

that produces mobile individuals tends to engender *both* sexual attitudes appropriate for higher social levels and relatively strong religiosity.

We will not comment here on the relatively more restrictive sexual attitudes and behavior of the women in the sample, other than to note that this seems to be in part a reflection of the double standard of sexual morality accepted by many persons in American society.

COMMUNICATIONS

ON VALUES AND VALUE THEORY

To the Editor:

Catton's, "A Theory of Value" (*American Sociological Review*, 24 [June, 1959], pp. 310-317) starts off with an incorrect assumption: "Every personality . . . is confronted at frequent intervals with the task of choosing among alternative desiderata" (p. 310). Actually man is continually, without any interruption during his whole lifetime, choosing among "alternative desiderata." Since these choices are observably manifested in behavior—and only in behavior—we may say that he is continuously choosing among alternative behavior items and behavior clusters. In my article "The Value Concept in Sociology" (*American Journal of Sociology*, 62 [November, 1956], pp. 262-279), I said: "The valuations of individuals, singly, or in the aggregate, can be known only from their actions. . . . This view . . . which, in practice, equates 'value' with 'action' is supported by, but not dependent on, the assumption that whatever a person does is what he wants to do most under the given circumstances at the given moment" (p. 276). If the suggestion "that axiology can be defined as the science of preferential behavior" will "penetrate the confusion" (Catton, p. 311) then it must be taken seriously. The discussion of values must become a discussion of preferential behavior. Neither Catton nor Charles Morris ever accept this logical necessity.

Taking this approach, however, can we translate Catton's definitions and hypotheses from a value language into a behavior language? Instead of saying with him that "a value is a conception of the desirable which is implied by a set of preferential responses to symbolic desiderata," (p. 312) let us say that "a value is known if there is a general statement—inferred from observations—which indicates that a certain behavior sequence will probably occur under certain circumstances." No distinction between symbolic or non-symbolic objects of behavior, verbal or non-verbal behavior, or any other need be introduced at this point.

Instead of hypothesizing (1) that "socially acquired conceptions of the desirable (values) influence human choices among non-symbolic desiderata" (p. 312), we may propose that "there is some correlation between verbal be-

havior and non-verbal behavior which permits us to predict from one to the other and even to control one through the other with some statable degree of probability." The direction of prediction and of control remains open. There is no reason for assuming *a priori* that either type of behavior must precede the other. Also, if both go on through any given time span, a mutual influence is conceivable. It seems equally unnecessary to insist that ways of behavior in situations must be socially acquired to deserve being called values. In fact this assumption would be contrary to the original assumption that *all* behavior is evaluative. As sociologists, we like to smuggle the word "social" in wherever there is a chance. But we know that practically all our behavior ("values") is socially learned, and shall let it go at that. We may state, however, an additional hypothesis: "There are specifiable situations in which people who have experienced certain specifiable kinds of interaction behave differently from those who did not experience them."

Hypothesis 1-a introduces a new concept: "Significant correlations may be found, at any given time, between values and personal desires" (p. 313). Other than socially acquired values apparently are to be called "desires." If both desires and values are known only in their manifestations as behavior, hypothesis 1-a merely states that "there will be some predictability between some of the different behavior clusters of the same person." This will hardly be doubted.

Hypothesis 1-b states that "within an isolated social system, such correlations tend to increase through time. That is, there is a strain toward alignment of desiring with socially acquired values" (p. 313). This may be read to say that "predictive probability between the behavior clusters of the same person will increase if no new circumstances interfere, particularly if there is social pressure for such consistency."

Hypothesis 1-c, "The influence of values . . . upon human choices among non-symbolic desiderata is conditioned by socially acquired knowledge of the characteristics of the desiderata" (p. 313), may be read as "probabilities that different kinds of behavior will occur vary in relation to the amount and kind of information which the actor has concerning the implications of these kinds of behavior." Why must the knowledge be socially acquired? Of course,

most of it probably is. Knowledge regardless of its manner of acquisition may be expected to have some effect on behavior.

Hypothesis 2: "When values are held constant, desiring (or 'motivation') varies inversely with the 'distance' (in an n-dimensional psychological space, or value-space) between the valuer and the desideratum" (p. 314). Translation: "Efforts required for different kinds of behavior are among the conditions and circumstances under which certain behavior clusters are more or less likely to occur than others." Subjective effort components are difficult to isolate from others. But in statistical studies correlations between added objectively equal amounts of resistance and differences in behavior may be obtainable. The overcoming of obstacles—up to a point—may, however, be sought as such, as in sport or in sexual conquest. The term "holding values constant" has no meaning if value is equated to preferential behavior and this behavior is expected to vary.

Hypothesis 3: "When values and desideratum-to-valuer distances are held constant, desiring varies with the activation of levels in some prepotency hierarchy . . . the intensity of a person's desire for a given object varies systematically from time to time, and . . . this intensity at any particular time is a function of the continuously perceived similarity of the object to other objects strongly desired at that time" (p. 315). Translation: "Sets of circumstances can be specified under which the probability of one kind of behavior will be higher than that of others. If some behavior is likely to occur under certain circumstances the probability of its occurrence under similar circumstances can be ascertained." The phrase "activation of levels in a prepotency hierarchy" is [not translatable] into any language related to observation.

Hypothesis 4: "A valuer's response to sets of subsidiated desiderata are more predictable than his responses to sets of independent desiderata" (p. 316). Translation: "If a behavior cluster which is likely to occur under certain circumstances is generally followed by a further behavior cluster, the result of the first making the second possible, the probability of the joint occurrence of both clusters is higher than the probability of joint occurrence of any two not so related clusters."

Hypothesis 5: "A valuer's responses to sets of congruent desiderata are more predictable than his responses to sets of independent desiderata" (p. 316). Translation: "If there are several sets of circumstances in which people are likely to behave in approximately the same manner or in manners which, though different, are compatible with each other, then if several of these sets of

circumstances coincide the probability of the occurrence of any such behavior is higher than if several coinciding sets of circumstances are not so related."

Hypothesis 6 finally reads: "When values are held constant, the order of preference among a set of desiderata may nevertheless vary from person to person or from group to group as a result of the failure of each person or group to be fully cognizant at all times of all dimensions of value-space" (p. 317). If values are defined as preferential behavior, then "holding values constant" means that the same preferences are exercised in behavior, in other words, the behavior is the same. The hypothesis may be restated as follows: "In situations for which the same behavior is predicted on the basis of past observations, observed behavior may actually differ from person to person or from group to group. Such differences from expectation are related to total or partial ignorance of the situation on the part of the actors." One might wish to add: "Some difference of the observed from the expected behavior may be due to circumstances to which the observer didn't, but the actors did pay attention."

The translation of the hypotheses into a behavior language makes it possible to forego such pained physicalisms as "n-dimensional value-space" or "value-field" and such avoidable complications as the distinction between value, desire, apparent value, etc. No doubt, human behavior is complex and difficult to predict. This complexity can be increased indefinitely by approaching it with conceptual tools that are more and more distant from and less and less related to actual observable behavior. It can be limited by using concepts which refer directly to it.

FRANZ ADLER

University of California, Davis

REPLY TO ADLER

When Adler asserted, several years ago,¹ that the "natural science sociologist . . . has no real need for a value concept," I was startled. I had felt a strong commitment to sociology as a natural science, but I could not agree that the value concept was superfluous. While I did agree that "valuations . . . can be known only from . . . actions," I did not follow Adler's leap to the conclusion that "values" can be "equated with action."²

¹ Franz Adler, "The Value Concept in Sociology," *American Journal of Sociology*, 62 (November, 1956), p. 275.

² *Ibid.*, pp. 276-277.

In my paper on "A Theory of Value," I tried to make careful distinctions between several interrelated terms: "value," "valuing," "desideratum." In his comments on my paper it is apparent that Adler has neglected these distinctions. It is his privilege to define and use these terms as he prefers in his own writing, just as it was my privilege to set forth definitions I considered useful at the beginning of my paper. But he should not read my paper using his definitions. If his definition equates "value" with "action" he will not understand hypotheses involving the word "value" where that word is intended to mean "a conception of the desirable which is implied by a set of preferential responses to symbolic desiderata."

He has similarly missed the significance of the contribution made by Morris, who has actually provided an operational definition of "conceived values" with which the "natural science sociologist" can work. People's values are not the objects they desire, nor even their desires for those objects, nor their strivings (actions) for those objects—as Morris defines "values." In *Varieties of Human Value*, Morris performed a factor analysis of a correlation matrix based on the ratings people had assigned to 13 "ways of living." The assignment of ratings was an action which expressed preference. To be sure, the factor matrix could only be known from these actions, but the factors obtained cannot in any sense be equated with the ratings. The factors constitute, in my view, the nearest empirical referent for "values."

Having neglected to learn the "language" in which my paper was written, it is curious that Adler should have labeled his substitute propositions "translations" of my hypotheses. It is particularly curious when he explicitly denies any meaning to the phrase "holding values constant" and yet proceeds to "translate" three of my hypotheses which involved that phrase. In one of these attempts he mistakenly equates my Hypothesis 2 with a sort of least-effort principle and then points out exceptions to such a principle. A careful reading of my discussion of the concept of "value-perspective" ought to have prevented this.

Rather than having "translated" my hypotheses into a "language related to observation," Adler has simply disconnected them from each other and from the value concept and has left us with a number of propositions which might turn out to be true but would not be very informative. For example: "Sets of circumstances can be specified under which the probability of one kind of behavior will be higher than that of others. If some behavior is likely to occur under certain circumstances the probability of its occurrence under similar circumstances can be

ascertained." This is little more than an articulation of the faith all of us have that sociology is possible; it certainly is not a translation of my Hypothesis 3, as it purports to be.

Proceeding from such a faith, let us make some observations. In a certain population we find a four-year cycle in the proportion of vituperative discussion conveyed by the mass media. This proportion rises to a peak around the end of October each leap year. Early in November it virtually disappears, to be replaced by messages of congratulations from one small group of persons to another and by innumerable analyses (showing little agreement with each other) as to why these congratulatory messages flowed from left to right, say, rather than from right to left as they had four years earlier. Suppose we have recorded a number of successive cycles of this type. We predict their continued recurrence. Why? Because we *infer* that the American people desire to uphold the Constitution and conform to the expressed will of the electorate more than they desire the election of their particular candidates (that is, we assume the efficacy of certain *values*—conceptions of the desirable—in the American political system).

I acknowledged in my paper that in studying values, a sociologist studies "inferential constructs" rather than directly observable phenomena. These constructs must, of course, be inferred from preferential behavior. What Adler suggests is simply this: the constructs are not worth the effort of inferring them. I contend that they are.

Adler's prescription for a healthy science is reminiscent of the old-fashioned almanac. Its daily weather forecasts (a year in advance) were simply assertions that the weather most often observed on a given date in past years would be the most probable weather on the corresponding date in the coming year. We could, if we wished, build an almanac-like sociology. Or, we could emulate the meteorologist who tries to ascertain the forces which produce specified kinds of weather. His forecasts tend to be more accurate (and more specific) than those of an almanac, and even when they err we still regard them as more sophisticated.

Adler's comments clearly imply that sociology must concern itself always with empirical regularities. I agree. It does not follow that we must *confine* our work to discerning and recording such empirical regularities. How are we to know that they will *remain* regular?

The sun, moon, and planets exhibit patterned movements in the sky, and man has (I presume) nearly always expected these regularities to persist. I doubt if he has often been content to leave it at that, however. He has sought to

"explain" the observed regularities. His explanations have varied from time to time. Theologically oriented people have attributed such regularities to Divine Providence. Since Newton we have attributed the recurrences to movements of the Earth and other planets in orbits—under the influence of gravity. Gravity is, however, an inferential construct. It is not directly observable; only the movements of falling objects or of celestial bodies are observable. Nevertheless, the gravity concept has been of great importance in the advancement of science.

We could continue to specify, as Adler would have us do, those "situations in which people who have experienced certain specifiable kinds of interaction behave differently from those who did not experience them." We might produce a large catalog of rather clear empirical regularities. I believe, however, that the value concept as I have defined it can enable us to go beyond this. By knowing something about the forces that produce observed regularities we know how to go about looking for previously unobserved patterns. We know how to relate one regularity to another.

Parsimony is not served by voiding inferential constructs but by choosing them wisely so that they can subsume many empirical regularities in one theory.

WILLIAM R. CATTON, JR.

University of Washington

ON "DEMOCRACY AND WORKING-CLASS AUTHORITARIANISM"

To the Editor:

A methodological problem arises in Mr. Lipset's article.¹ He states: "the results [of a UNESCO study in which Germans were asked: 'Do you think that it would be better if there were one party, several parties, or no party?'] analyzed according to occupational status indicate that the lower strata . . . were less likely to support a multi-party system" (p. 485). Necessary to this induction's validity is the unwarranted assumption that upper- and middle-class (UMC) Germans answered this question in good faith. They might have conceivably been sophisticated enough to realize that authoritarian expressions are contrary to contemporary mores. Confidentiality of a questionnaire does not necessarily immunize it from respondents' doubts. Their responses by no means preclude the probability that authoritarianism is rela-

tively equally distributed among [German] classes.

Whereas my doubt concerns the case of the Germans, Lipset's *general* thesis, when "other things (such as historico-cultural factors) being equal," is not unacceptable.

The article, however, raises a more serious question, from the angle of the relation between Lipset's findings and his ontological orientation. He states (p. 482) that intellectuals of the democratic left (IDL), who once believed the proletariat to be a force for liberty and progress, are posed with a "tragic dilemma" (in view of the facts of working-class authoritarianism). I submit that while Lipset's findings may be correct, he uses them unduly to support his ontological notions. IDL conceived of liberty and progress as liberation from the situation in which the proletariat was dominated by other classes. Today, such a domination varies, depending on socio-historical developments. Even in progressive capitalistic countries, where this domination is decreasing, it is accompanied by painful frictions, for whose containment the "proletariat" struggles for equalizing measures, while other classes presumably struggle for the preservation of the *status quo*. Lipset's statement that "the upper classes resisted the extension of political freedom as part of their defense of economic and social privilege" (p. 499) is inconsistent with the distinction between economic and non-economic liberalism, which he makes earlier (p. 485). Furthermore, IDL do not deny that progressive elements exist in the middle class.

If Mr. Lipset regards the IDL's notions of progressive force as holding in the *existing* order, then not only would it backfire at his argument with regard to IDL, but even with regard to the communist party itself! For he suggests that [this] party in modern democratic societies, lacks no complex frame of reference (p. 497). Thus, what he regards as an intervening variable between low status and authoritarianism is absent even in the communist party in democracies, as he admits, let alone among leftist democrats!

On the other hand, if he refers to the IDL's notions of progressive proletarian gains in a *classless* society, he has no logical basis on which to put IDL in a "dilemma" (since difference between classes is nonexistent, by definition, in a classless society).

As to the bitterness and insecurity of the poor in the existing order, I know of no IDL who has ever disputed it; on this very fact Marxist philosophies are based. Mr. Lipset regards economic and psychological insecurity as a contributing factor [to] authoritarianism. It is a real question, which should be empirically

¹ Seymour Martin Lipset, "Democracy and Working-Class Authoritarianism," *American Sociological Review*, 24 (August, 1959), pp. 482-501.

answered, as to who is more insecure: a worker who gets his regular pay-check and who is backed in times of crisis as well as in normal times—by a mighty union, or a businessman who has unexpected ups and downs, . . . may become bankrupt within a short time, and . . . fears the labor unions. One cannot prove the unprovable. When Mr. Lipset tries empirically to support his ontological notions, he [has] logical difficulties. When he states the truism that the social situation of the lower strata predisposes them to view politics in black and white terms, he is forced to qualify his statement [by referring] to "particularly in poorer countries with low levels of education" (p. 483), which puts him in deep water relative to his thesis that "authoritarian predispositions . . . flow more naturally from the situation of the lower classes than from that of the MUC in *modern industrial society*" (p. 482, *italics mine*), i.e., in societies which are not "particularly poor."

Furthermore, Mr. Lipset admits that "commitments to democratic procedures . . . by the principal organizations to which low status individuals belong may influence their actual political behavior more than their underlying authoritarian personal values" (p. 484). If so, on what basis does he assert that IDL are confronted with the above dilemma? Mr. Lipset would admit that when IDL refer to the proletariat as a progressive force, they are considering, indeed, a *social force*, which can be progressive regardless of the conservatism of its individuals. If the postulate that "the group is more than the mere sum of its individuals" is correct, there is no incompatibility between authoritarianism of *workmen* and progressiveness of *working class*.

Among the elements contributing to authoritarianism, Mr. Lipset suggests isolation of occupation. But this very element is crucial for supporting the argument of IDL, whose belief Lipset regards as dilemmatic. Precisely because in a leftist-conceived society it is the workmen who are *not* isolated, they should not be expected to be prejudiced. The concept of proletariat in leftist-conceived society implies no isolation.

Mr. Lipset speaks of (lower-class) people as uprooted from a stable community life (p. 491). But is this not equally true of mobile professors, who are typical of the United States scene? Yet are these professors as authoritarian? This problem could be solved, had Mr. Lipset taken the pains to specifically define the concept 'stable community life' in cultural terms—which he has failed to do. Instead, he merely talks of "rootlessness flowing from rapid industrialization."

Mr. Lipset says: "The American labor movement has opposed non-white immigration . . ." and "the Marxist Socialist movement . . . was not immune to anti-Semitism" (p. 499, n.). Before inferring his thesis from [such] facts, he should have proved that, *compared to non-labor and non-Marxist movements*, labor movements are *more* anti-Semitic. His assertions that "In spite of the workers' greater authoritarian propensity, their organizations which are anti-communist still function as better defenders and carriers of democratic values than parties based on the middle class" (p. 500), and that "it would be a mistake to conclude . . . that authoritarian predispositions of the lower classes necessarily constitute a threat to a democratic social system" (p. 500) [do not warrant] his presumption concerning the dilemma of IDL.

To the extent that personal convictions, legitimate as such, sanctify findings, a single ontological statement, out of many empirical ones, may carry their carriers out of their way, so as to render their inductions invalid.

Finally, to say: "whether democratic working-class parties able to demonstrate convincingly their ability to defend economic and class interests can be built up in the less stable democracies is a moot question" (p. 501) is ontological. We know that whether or not democratic political groups can exist in *any* society is a moot question. It was once said: "Man is a contradictory being. He desires freedom and power, but the drive for power leads to conflict with others. It tends to narrow the freedom of others."²

Rather than forcing social relationships to preconceived dichotomies, or disguising them, our task is to determine the extent to which they exist, and the features in which they are manifested.

ELKHANAN LAHAV

University of California, Los Angeles

REPLY TO LAHAV

To the Editor:

I have read the comments by Mr. Lahav on my article on "Democracy and Working-Class Authoritarianism" with great interest. Although his faith in the nature of lower-class based politics may remain firm, it is obvious that many intellectuals of the democratic left, among whom I number myself, have been disturbed by the sorts of data I cite in the article. For ex-

² Frank T. Carleton, "Toward Peace Between Nations and Within Nations," *Sociology and Social Research*, 40 (November, 1955), p. 103.

ample, Ignazio Silone perhaps Italy's leading socialist writer, has publicly stated his lack of faith in the democratic potential of the working class; Gino Germani, now professor of sociology at the University of Buenos Aires, who has spent decades opposing Fascism and Peronism, first in Italy and later in the Argentine, has written a number of articles documenting the popular base of authoritarianism; Lewis Feuer in the introduction to his newly published collection of the sociological and political writings of Marx and Engels poses the same problem when he points to the fact that, "The history of the masses . . . has been a history of the most consistently anti-intellectual force in society . . . [that] it was the American lower classes not the upper who gave their overwhelming support to the attacks in recent years on civil liberties;" and articles published in the left socialist magazines *Dissent* and *The New Statesman* have called attention to the problem. A variety of evidence, none of which Mr. Lahav challenges, suggests that the lower strata are more intolerant and are less likely to support democratic rules of the game than are the middle classes. Demagogic extremist politicians such as the Communists, McCarthy, and Peron have secured considerable strength from the lower strata by combining political intolerance with attacks on social and economic elites.

Mr. Lahav makes some specific criticisms. . . . First, I obviously cannot prove that the German data I cited are not the product of conscious distortion by upper- and middle-class Germans. However, other studies in Germany give the same results. And the very fact that the pattern of opinion in Germany is similar to that found in many other countries suggests that the German results are valid. Second, although I suggest as Mr. Lahav properly points out that the lower strata are more likely to be extremist or intolerant in "poor countries with low levels of education," this does not mean there is no problem in wealthy well-educated countries. The evidence indicates that even in such nations the lower strata and the less educated are much more likely to be intolerant than those who are more well-to-do and better educated. The American data cited in the original article demonstrate this clearly. And recent British survey findings indicate that when voters were asked whether they would vote for a candidate of their own party who is Negro or Jewish, workers were more likely to say that they would not vote for a Jew or a Negro than were middle-class respondents. In talking with important members of the British Labour Party last summer I found that they were aware of this fact and were convinced that agitation by the party on the subject of Negro rights, either

in London or in central Africa, though official party policy, does not do the party much good with the working-class part of the electorate.

Mr. Lahav, I think, misunderstands my point concerning the democratic political role of the socialist and labor movements. I contended that the non-economic liberalism of these movements on issues such as civil liberties, civil rights, and foreign policy, basically reflect the values of middle-class intellectuals and are imposed on the working-class adherents of these parties. The "natural" attitudes of the lower strata derivative from their social situation include antagonism to elites, xenophobia, and intolerance. These groups may be recruited to political movements which fight against the existing distribution of privilege and by so doing seem to accept liberal values on non-economic issues since these are less salient to them. A look at public opinion data from different countries indicates clearly that for those with little education and socio-economic status issues which do not touch on specific self-interests are of little concern on most occasions. However, in those situations in which intellectuals play little role as lower-class leaders, the labor movement is more intolerant than the middle-class organizations. For example, in Australia the Labor Party has been a stronger advocate of the "white Australia" policy than the middle class conservative parties have been. In the United States the labor movement was not interested in Negro rights until recently, and for that matter neither were the Democratic or the Socialist parties. If one examines the history of the Socialist Party in the United States before World War I, when it had considerable working-class strength in many areas, it is clear that the party not only did not actively support equal rights for Negroes but its two mass circulation newspapers, *The Appeal to Reason* and the *Milwaukee Herald*, both contended that socialism would not mean social equality.

Mr. Lahav suggests that the conditions I discuss will not exist in a classless society. I cannot share his belief that such a society is feasible in any meaningful sense, but I regretfully must note that even in those existing societies dominated by democratic socialist movements intolerance towards minority groups continues as a strong aspect of the system. Current Swedish survey data, for example, indicate a high level of popular anti-Semitism after a quarter of century of Social-Democratic government. And communism has a much worse record in Russia where the vicious anti-Semitic policies of Stalin have been modified but not eliminated by his successors.

A leftist intellectual should not be upset to learn that poverty, insecurity, and ignorance do

not produce as "decent" people as do wealth, security, and knowledge. But it is true that recognition of these facts poses problems for those who also believe that political movements reflect the social position and values of the people on whom they are based.

SEYMOUR MARTIN LIPSET
University of California, Berkeley

ON CULTURE AND PERSONALITY

To the Editor:

In a review of my brief essay, *Culture and Personality* (*American Sociological Review*, 24 [June, 1959], p. 430), Endleman makes some adverse judgments . . . , then demonstrates that he has failed to grasp its central intent and that he has made the following erroneous and misleading observations:

1. This essay is not intended as a comprehensive critique or integration of the literature—the criteria by which he judges it. Instead, it aims to clarify the approaches of four behavioral disciplines concerning the cultural dimension of personality and the psychological facets of group behavior. And it presents some representative problems to illustrate these positions.

2. He mistakes a disciplinary approach for a conceptual approach; then contends that differences *within* any one discipline are as marked as differences *between* disciplines. This inference, which is definitely inaccurate, ignores the centripetal forces within each discipline which compel each specialist to conform to his particular professional role in his inquiries.

3. He neglects to mention the essay's treatment of the intricately and diversified multidisciplinary cross-cultural treatment of the effects of early life upon later behavior, ranging in psychoanalysis from Roheim's rigid theory of early influences upon erogenous zone behavior to Kardiner's and Fromm's approach, and in social science from Orlansky's critique to Kluckhohn's careful observations in the Navaho society and to Sewell's empirical analysis. Instead he dismisses it as "crudely defined."

4. He confuses inter-disciplinary convergence for inter-disciplinary integration. For example, in the above-mentioned problem, . . . some sociologists acknowledge the importance of early life experiences upon subsequent behavior, while some psychoanalysts have acknowledged the importance of experience after childhood in personal breakdown. This convergence does not mean integration. And it does not mean that an over-all coherent theory exists, as he implies.

5. He uses this non-existent inter-disciplinary theory as a criterion for devaluating works

which recognize and must cope with the extant inter-disciplinary realities. Whether or not he is self-deluded into clinging to this utopian inter-disciplinary theoretical synthesis is subsidiary in importance to the fact that he misleads the reader into believing that this represents a legitimate criterion for evaluating inter-disciplinary inquiries.

6. Since he mistakes a conceptual approach with an integral disciplinary approach, he feels that the conceptual and technical aspects should be separated. Although we have devoted a specific section to methodological problems which he neglects to mention, we emphasize that the conceptual problems are integral aspects of a total methodology, and that the type of concept contains within itself an implied method, such as the loose clinical concepts of the psychoanalysts or the rigorous operational concepts of the experimental psychologists' pattern. Thus we reiterate that the conceptual and technical aspects do not have to be separated.

7. He mentions that particular works by Merton and LaBarre have been omitted. Articles of both authors have been mentioned when necessary, and the works to which he refers are not essential to this presentation despite their intrinsic merit. His reference to these works is consistent with his general misunderstanding of the essay.

S. KIRSON WEINBERG
Roosevelt University

REPLY TO WEINBERG

To the Editor:

In reply to Mr. Weinberg's communication: readers may consult his essay and my review, and decide for themselves. Cramped for space, I limit myself to a few incidental comments here:

Mr. Weinberg's self-set task *required* a critical review of literature, which he in fact undertook. I quarrel with the *terms* in which he did so: the arrangement by disciplines. These historically accidental agglomerations of activities do exist, of course, and do exert centripetal pressures, as Weinberg says. Their boundaries have also hindered good conceptual formulation of culture and personality problems. I do not identify a conceptual approach with a disciplinary one. In distinguishing them, I prefer the former as a means of defining problems for theory and research. Example: Parsonian action theory. This seeks to identify and interrelate problems of personality systems, social systems, and cultural systems within a single consistent theoretical frame. This does not correspond to currently prevailing

boundaries among disciplines. A Parsonian might ask to redefine the boundaries of, say, psychology, sociology, and cultural anthropology, in terms of this tripartite division, but how many practitioners will agree to being sorted out in just this way? Endless and futile boundary-setting wars ensue. Better encourage scholars of whatever provenience to "try on" such available models to look at some phenomena they are interested in, and try thus to generate propositions relatable to general theory. Many behavioral scientists at present work essentially in this way, treating their disciplinary tags as institutional convenience or not. Examples are Cohen on delinquent boys and Vidich and Bensman on the small town in mass society. Both make important propositions relating personality and social system, propositions meaningful in each case within a larger body of theory. Critical papers presenting an alternative to Weinberg's emphasis include Inkeles' "Personality and Social Structure" in *Sociology Today*, to give just one example. As for "interdisciplinary theory," which Weinberg finds "non-existent," may I nominate two different attempts: (1) the Parsons-*et al.* scheme just mentioned; (2) Weston LaBarre's *The Human Animal* mentioned in the review, which essays an integrated and systematic ordering of propositions derived from many different disciplines. If being encouraged by such boundary-leaping recent work means being "deluded" and "utopian," then I accept the epithets.

ROBERT ENDLEMAN

State University College on Long Island

ON THE MODIFICATION OF KENDALL'S COEFFICIENT

To the Editor:

Just for the record, I would like to call attention to the fact that, in his modification of Kendall's coefficient of concordance to fit sociometric data, James A. Jones (in "An Index of Consensus on Rankings in Small Groups," *American Sociological Review*, 24 [August, 1959], pp. 533-537) has been anticipated by at least two other writers. F. Kräupl Taylor derived the same formula as early as 1951 (*Journal of Mental Science*, 97, pp. 690-717), and Ben Willerman independently arrived at the same result in 1955 (*Psychological Bulletin*, 52, pp. 132-133). Taylor provided a correction for tied ranks, and Willerman presented a table of significant values for groups of size 4 to 15.

It is, perhaps, not especially important to establish who was first and who was not in such matters, but instances of this kind under-

score the ever-increasing burden placed upon the specialist who desires to keep up with developments in his field. The exponentially rising rate at which printed material is turned out threatens to inundate us, and it seems high time for some mighty mind to devise a real solution to the problem. Or perhaps such a solution already exists somewhere among the tens of thousands of books and journals making up the scientific literature of recent years. But—how to find it?

RICHARD H. WILLIS

Carnegie Institute of Technology

REPLY TO WILLIS

To the Editor:

Thanks are due Professor Willis for filling us in on the disjointed history of Kendall's coefficient of concordance. Knowledge of related work is essential in all scientific endeavors.

My effort, in contrast to those of Taylor, Willerman, Durbin, and others, was directed primarily to showing that it *does* make a difference which coefficient of concordance is used. Much of the work in the area of small groups implies that a person can gauge fairly accurately his position in the group or his contribution to completion of the group task. The wide discrepancies between W and W', however, indicate that this is not always the case. I regret that this point of the article failed to come through as clearly as it should have.

Every sociologist can sympathize with the closing sentiments of Professor Willis. It is well nigh impossible to "keep up" with all of the literature on a given topic, for relevant materials appear in so many diverse publications. In view of the situation, the chief virtue of the article (and it's a kind of solution to the problem) may be that it appears in the *American Sociological Review*.

JAMES A. JONES

New York School of Social Work

ON RECIPROCAL INHIBITION

To the Editor:

When a book espouses a thesis that conflicts with the psychoanalytic Zeitgeist, hostile reviews are only to be expected. However, when an antipathetic reviewer seems not to have been able to bring himself to read the book in question, it is time to protest. Accordingly I wish to take issue with Thomas F. A. Plaut regarding his review of my book *Psychotherapy by Re-*

ciprocal Inhibition (the Review, February 1959).

Several of Plaut's statements testify to the haziness of his acquaintance with the book. To begin with, he states that I emphasize *retroactive inhibition*, when in fact this kind of inhibition is mentioned only once—in rather an incidental way. Then he alleges that the "main responses that Wolpe seeks to develop are assertive and sexual responses." I do not "develop" these responses but only *utilize* them (and even more often utilize relaxation responses) because of their anxiety-inhibiting effects. Plaut further alleges that "In some cases a mild electric shock or hypnotism is used to evoke the required motor response." Nowhere does the book describe hypnotism being used to evoke motor responses, and electric shocks are used only in very unusual cases. Motor responses *as such* are seldom "re-

quired." Misstatements of this kind would not have been possible if Plaut had read the book with reasonable diligence.

J. WOLPE

Johannesburg, Union of South Africa

REPLY TO WOLPE

To the Editor:

Dr. Wolpe is absolutely correct in picking up my error in stating that his book emphasizes *retroactive* inhibition. Clearly, as the title of the volume indicates, the review should have referred to *reciprocal* inhibition. I regret this error.

THOMAS F. A. PLAUT

Harvard School of Public Health

Announcing . . .

THE MacIVER LECTURESHIP, 1960-1961

The MacIver Lectureship will be awarded in August 1960 to the author or authors of a publication which, in the opinion of the Selection Committee, contributed outstandingly to the progress of Sociology during the two preceding years.

The recipient will undertake to deliver a public lecture at a meeting of an affiliated regional society other than that of his own region.

The MacIver Lecture will be submitted for publication in the *American Sociological Review*.

The recipient will be given a stipend of 500 dollars from the MacIver Fund, and a travel allowance equal to the round-trip first-class railroad or airplane fare between his home and the place of the lecture.

Members of the American Sociological Association are invited to suggest candidates for consideration by the Selection Committee. In making suggestions, members should have in mind MacIver's special interest in theory, but need not feel bound by this emphasis. Suggestions should be sent not later than March 1, 1960, to the Chairman of the Selection Committee or to the Executive Officer, American Sociological Association.

The Award will be announced at the annual meeting,
August 29-31, 1960, New York, N. Y.

SELECTION COMMITTEE

Harry Alpert, Chairman, University of Oregon
John A. Clausen, National Institute of Mental Health
W. Rex Crawford, University of Pennsylvania
Robert K. Merton, Columbia University
Stuart A. Queen, University of Wichita
William H. Sewell, University of Wisconsin

THE AMERICAN SOCIOLOGICAL ASSOCIATION
New York University
Washington Square, New York 3, N. Y.

THE PROFESSION: REPORTS AND OPINION

STATE REPRESENTATIVES OF THE COMMITTEE ON THE PROFESSION

The Sub-Committee on Legislation that Certifies Psychologists (David F. Aberle, Harold L. Wilensky, and Guy E. Swanson, Chairman) reports that the Board of Directors and Council of the American Psychological Association (APA) adopted the legislative proposals suggested by the American Sociological Association (ASA). These proposals, as presented in the *Review* (June, 1959, p. 402) and in *The American Psychologist* (October, 1959, pp. 666-667), are now official APA policy, binding on state psychological associations.

Members of the ASA are asked to report to their respective Representatives, listed below, any developments within the state which might have implications for sociologists (such as proposed legislation on certification, teacher training, and civil service requirements).

Robert E. Garren, 118 South Ross Street, Auburn, *Alabama*
 Clyde B. Vedder, Dept. of Sociology, University of Arizona, Tucson, *Arizona*
 Stephen Stephan, Dept. of Sociology & Anthro., University of Arkansas, Fayetteville, *Arkansas*
 C. Wilson Record, Dept. of Sociology, Sacramento State College, Sacramento 21, *California*
 Oliver Whitley, Iliff School of Theology, Denver, *Colorado*
 Jerome K. Myers, Dept. of Sociology, Yale University, New Haven, *Connecticut*
 Irvin W. Goffman, Dept. of Sociology, University of Delaware, Newark, *Delaware*
 John A. Clausen, Nat. Inst. Mental Health, U.S. Publ. Health Serv., Bethesda 14, *Maryland (District of Columbia)*
 Bryce Ryan, Dept. of Soc. & Anthro., University of Miami, Coral Gables 46, *Florida*
 Chandler Washburne, 110 Peabody Hall, University of Florida, Gainesville, *Florida*
 Raymond Payne, Dept. of Sociology, University of Georgia, Athens, *Georgia*
 Andrew W. Lind, 2609 Doris Place, Honolulu 14, *Hawaii*
 Harry C. Harmsworth, Dept. of Sociology, University of Idaho, Moscow, *Idaho*
 J. E. Hulbert, Jr., Dept. of Sociology, University of Illinois, Urbana, *Illinois*
 Gerald R. Leslie, Dept. of Sociology, Purdue University, Lafayette, *Indiana*
 Harold W. Saunders, 127 MacBride Hall, State University of Iowa, Iowa City, *Iowa*
 Carroll D. Clark, Department of Sociology, University of Kansas, Lawrence, *Kansas*
 Robert I. Kutak, Dept. of Sociology, University of Louisville, Louisville, *Kentucky*
 Vernon J. Barton, 150 Himes Hall, Sociology Department, Louisiana State University, Baton Rouge, *Louisiana*
 Warren Breed, Dept. of Sociology, Newcomb College, Tulane Univ., New Orleans, *Louisiana*
 Theodore Weller, University of Maine, Orono, *Maine*
 Leila C. Deasy, National Inst. of Mental Health, Room 1949, HHH, Bethesda, *Maryland*
 Alex Inkley, Harvard University, Cambridge, *Massachusetts*
 Leonard Moss, Wayne State University, Detroit 2, *Michigan*
 Henry Riecken, University of Minnesota, *Minnesota*
 Marion T. Loftin, Dept. of Sociology & Rural Life, Box 156, State College, *Mississippi*
 Noel P. Gist, Dept. of Sociology, University of Missouri, Columbia, *Missouri*
 Barbara R. Day, Dept. of Sociology, Montana State University, Missoula, *Montana*
 Alan P. Bates, Social Science Building, University of Nebraska, Lincoln, *Nebraska*

Carl W. Backman, Dept. of Sociology, University of Nevada, Reno, *Nevada*
 Francis Merrill, Dept. of Sociology, Dartmouth College, Hanover, *New Hampshire*
 Jackson Toby, Dept. of Sociology, Rutgers, The State University, New Brunswick, *New Jersey*
 Sigurd Johansen, New Mexico College of A & M A, State College, *New Mexico*
 Edgar F. Borgatta, Department of Sociology, Cornell University, Ithaca, *New York*
 Charles E. Bowerman, Dept. of Sociology, University of North Carolina, Chapel Hill, *North Carolina*
 Robert B. Campbell, 619 Park Drive, Grand Forks, *North Dakota*
 Ray Mangus, Dept. of Sociology, Ohio State University, Columbus, *Ohio*
 Salomon Sutker, Dept. of Soc. & Rural Life, Oklahoma State University, Stillwater, *Oklahoma*
 James L. Price, 955 East 24th Ave., Eugene, *Oregon*
 Ray H. Abrams, University of Pennsylvania, Philadelphia, *Pennsylvania*
 James D. Thomson, Adminstr. Science Center, University of Pittsburgh, Pittsburgh 13, *Pennsylvania*
 Harold W. Pfautz, Brown University, Providence, *Rhode Island*
 Dorothy Jones, Box 42, Winthrop College, Rock Hill, *South Carolina*
 Carol M. Mickey, Dept. of Sociology, State University of South Dakota, Vermillion, *South Dakota*
 Edward McDill, Dept. of Sociology, Vanderbilt University, Nashville, *Tennessee*
 Gideon Sjöberg, Dept. of Sociology, University of Texas, Austin, *Texas*
 Henry H. Frost, Jr., Dept. of Sociology, University of Utah, Salt Lake City, *Utah*
 Paul Oren, Dept. of Soc. & Anthro., The University of Vermont, Burlington, *Vermont*
 Edward W. Gregory, Jr., University of Richmond, Richmond, *Virginia*
 Jack R. Parsons, School of Social Work, University of Washington, Seattle 5, *Washington*
 Harold Kerr, University of West Virginia, Morgantown, *West Virginia*
 Louis H. Orzack, Dept. of Soc. & Anthro., University of Wisconsin, Madison 6, *Wisconsin*
 Laurence Smardon, 1524 Ord Street, Laramie, *Wyoming*
 Oswald Hall, Dept. of Polit. Economy, Univ. of Toronto, 273 Bloor St. W., Toronto, Ontario, *Canada*

THE SOCIOLOGIST AS MEDICAL EDUCATOR: A DISCUSSION *

SAMUEL W. BLOOM, *Baylor University College of Medicine*, ALBERT F. WESSEN, *Washington University School of Medicine*, ROBERT STRAUS, *University of Kentucky College of Medicine*, GEORGE G. READER, M.D., *Cornell University Medical College*, and JEROME K. MYERS, *Yale University*

In recent years the number of activities available to the sociologist in the field of medicine has increased rapidly. The resulting contributions to research have been reviewed in detail by Caudill,¹ Freeman and Reeder,² Anderson and

* This paper is based on a panel discussion held at the annual meeting of the American Sociological Society, August, 1958.

¹ William Caudill, "Applied Anthropology in Medicine," in A. L. Kroeber, editor, *Anthropology*

Seacat,³ and Reader and Goss.⁴ The role of the sociologist as a medical educator, although one of the most radical departures from tradition, has not been subject to the same careful scrutiny. For this reason at the 1958 annual meeting of the American Sociological Society a panel was organized to discuss "sociology in medicine," defined as teaching and research undertaken in collaboration with medical colleagues by sociologists who hold faculty or staff status in organizations devoted to medical education.⁵ This paper is a summary of the panel discussion, prepared by the participants.

EXTENT OF FUNCTION AS MEDICAL EDUCATORS

According to the "List of Medical Sociologists" compiled annually by the Committee on Medical Sociology, 309 individuals currently define themselves as engaged to some extent in activities which include medical sociology.⁶ Of these, 224 are sociologists, including 11 who are both sociologists and anthropologists, 17 who are both sociologists and psychologists, and one physician-sociologist. The remainder are anthropologists, psychologists, physicians, and others more difficult to classify.⁷

Among those who are primarily identifiable as sociologists, 19 are full-time members of 14 dif-

ferent medical college staffs; in significant measure, they are directly responsible for teaching medical students. In addition, nine sociologists are members of the teaching faculties of five graduate schools of public health and one school of nursing. Ordinarily, full-time research appointments in a medical school involve at least indirect responsibility for teaching; there are 13 sociologists with such appointments in seven medical colleges. In four graduate schools of public health, nine sociologists have full-time research appointments.⁸

It is more difficult to classify sociologists who are part-time medical educators. According to the records of the Committee on Medical Sociology, 17 sociologists are engaged part-time in medical sociology and have some responsibility for teaching medical students. In addition, 57 sociologists do some teaching of other medical personnel. The remaining 100 sociologists on the List are classified in categories which have no known teaching function.

HISTORICAL ORIGINS *

The role of the sociologist in medical education cannot be fairly appraised without reference to its particular history and to the history of medical education as a whole. To many, the recent increased participation of social scientists in medical education represents a radical departure from tradition. Actually, this is not the case. Medical educators have experimented persistently with their curricula, thus demonstrating marked sensitivity to the changing demands of their profession. Their more recent innovations, including the expanded role assigned to sociologists, are part of a half-century old tradition of educational experimentation.

The present pattern of medical education may be said to have appeared on a large scale in the decade before World War I. At that time, the private proprietary school became obsolescent, and was replaced by an association between the professional medical school and the university, substantially following the model of German medical education. This model emphasized both the installation of full-time clinical faculties and the development of research-oriented basic science departments. It resulted in medical schools in which the laboratory became as much an intellectual reference point as the ward. Such

Today, Chicago: University of Chicago Press, 1953, pp. 771-806.

² Howard E. Freeman and Leo G. Reeder, "Medical Sociology: A Review of the Literature," *American Sociological Review*, 22 (February, 1957), pp. 293-303.

³ Odin W. Anderson and Milvoy Seacat, *The Behavioral Scientist and Research in the Health Field*, New York: Health Information Foundation, 1957.

⁴ George G. Reader and Mary E. W. Goss, "The Sociology of Medicine," in R. K. Merton, L. Broom, and L. S. Cottrell, Jr., editors, *Sociology Today*, New York: Basic Books, 1959, pp. 229-246.

⁵ Robert Straus, "The Nature and Status of Medical Sociology," *American Sociological Review*, 22 (April, 1957), pp. 200-204. In this paper, two major types of medical sociology are specified: the sociology of medicine and sociology in medicine. The former is "concerned with studying such factors as the organizational structure, role relationships, value system, rituals, and functions of medicine as a system of organized human behavior responding to a group of basic human needs."

⁶ See Straus, *op. cit.*, p. 200, for a description of the Committee on Medical Sociology. Inquiries may be addressed to S. W. Bloom, Corresponding Secretary, 2310 Baldwin, Houston, Texas.

⁷ The "List of Medical Sociologists" includes 33 anthropologists, 13 psychologists, 21 physicians, three physician-anthropologists, two physician-psychologists, one anthropologist-psychologist, and 12 others.

⁸ Some research appointments are in the same schools where major teaching appointments occur. Twenty undergraduate medical schools and seven graduate schools of public health have appointed one or more sociologists either as full-time teaching faculty members or as full-time researchers.

* By Albert F. Wessen.

a scientifically oriented institution perforce adjusted its curriculum as scientific advances were made. It soon became apparent, however, that a biochemically- and physiologically-based curriculum failed to provide sufficiently for the study of certain clinical problems. Much of the history of medical school curricula during the past 30 years can be written in terms of an expanded effort to deal with the social aspects of medicine, usually within departments of psychiatry or preventive medicine.

In a sense, it may be said that the very successes of scientific medicine made necessary the broadening of its perspectives. As headway was made against the acute illnesses, the chronic diseases, statistically, came to the fore. These long-term conditions, whether primarily physical or mental in character, were seen to involve long-term adjustments of the patient's social roles. Physicians found in chronic illness challenges which required application of knowledge about human motivation and social behavior for their solution. This fact was most evident in the case of the psychiatric illnesses, where social and behavioral dimensions were demonstrated to be integral to an understanding of etiology and therapy. Moreover, when psychodynamic concepts were shown to be germane to the understanding of a whole class of organic illnesses (the psychosomatic conditions), the relevance of the behavioral sciences to the understanding of human physiology and pathology was established in principle. Increased understanding of the etiology of disease—and the development of effective techniques of intervention—made prevention a possibility as well as a goal in medicine. But public health workers soon discovered that the successful introduction of preventive measures involved changing the customs and attitudes of whole communities. Thus, as the scope of medicine became more comprehensive in theory and practice, it increasingly encountered the domain of the social sciences.

Developments in medical science have also greatly altered the pattern of medical practice. Hospitals have become broader in scope and more complex in organization; specialization has threatened to fragment medical care as it has increased its technical efficiency; the number of health organizations and services in the average city has become legion. Individualistic patterns are being supplanted by collaborative ones: the emergence of the "team" concept in the hospital, the development of group practices in medicine, and the widespread diffusion of third-party payment plans are manifestations of a more general trend. Accordingly, understanding of complex organizational patterns is becoming

increasingly important for intelligent medical practice. In this way, too, the knowledge of the sociologist may prove useful in medical education.

That sociologists and others have become involved in new programs in medical education is thus in large part a result of the evolution of medicine itself. Changing conditions have made evident the following needs, which sociologists have been asked to help meet: (a) research concerning social factors in the etiology and treatment of disease; (b) training of administratively competent physicians who know the medical organizations and facilities in their communities; and (c) orienting physicians to recognize and deal with social factors in diagnosing and managing illness.

It may be significant that the introduction of social science in the medical curriculum has been most noticeable in departments which are generally low in status in the medical school. In this respect, as in others, the situation of the social scientist may be compared with that of the biological scientists when they were incorporated into American medical schools, beginning about 75 years ago. These scientists were brought into the medical school to aid in meeting the perceived needs of the day. They won acceptance only as they proved their ability to fulfill this mandate. So will it be with the social scientist in his ventures in medical education.⁹

⁹ Some sources dealing with the historical trends discussed in this section are: Abraham Flexner, *Medical Education in the United States and Canada*, A Report to the Carnegie Foundation for the Advancement of Teaching, Bulletin No. 4, New York, 1910; Raymond B. Allen, *Medical Education in the Changing Order*, New York: Commonwealth Fund, 1946; Report of the Joint Committee of the Association of Medical Colleges and the American Association of Medical Workers, *Widening Horizons in Medical Education*, New York: Commonwealth Fund, 1948; Lester J. Evans, "The Next Twenty Years in Medicine," *Journal of Medical Education*, 27 (September, 1952), pp. 326-329; Allen Gregg, "Transition in Medical Education," *Journal Assn. Am. Med. Coll.*, 22 (July, 1947), pp. 226-232. For a more general description of the medical school, see: John E. Dietrick and R. C. Berson, *Medical Schools in the United States at Mid-Century*, New York: McGraw-Hill, 1953; Robert K. Merton, Patricia Kendall, and George Reader, *The Student-Physician*, Cambridge: Harvard University Press, 1957; Helen H. Gee and Robert G. Glaser, editors, "The Ecology of the Medical Student," *Journal of Medical Education*, 33 (October, 1958), Part 2, particularly articles by Christie and Merton, Richmond, Reader, and Cooper and Harrell.

THE PRESENT POSITION OF THE SOCIOLOGIST
AS MEDICAL EDUCATOR *

The sociologist who is a teaching member of the faculty of a medical school is most likely to find himself in either the department of public health and preventive medicine or the department of psychiatry.¹⁰ Whatever the department, the ranks and salaries of sociologists compare favorably with those of their Ph.D. colleagues in the medical school. Like the latter, the sociologists' teaching responsibilities tend to be concentrated in the preclinical phase, regardless of specific departmental affiliations.¹¹ In other ways, however, the role seems to vary according to the departmental situation. This is particularly true of *what* the sociologist teaches and *how*.

What the medical sociologist teaches seems to fall into two major categories: population and demography, and social system theory.¹² Anderson has argued in favor of an emphasis on the former type of content.¹³ The model for the social system approach may be found in papers by Henderson¹⁴ and Parsons¹⁵ on the doctor-

patient relationship. "Social system theory" is probably a premature term. More accurately, it may be called a "conceptual frame of reference" which focuses on a "social actor" whose behavior is significantly determined by his membership in various sub-cultures and social institutions.

Views on how sociology should be taught in medical school also may be divided into two main categories. The first is in the tradition of "learning by doing," as, for example, in the Family Health Advisor Service of the University of Pennsylvania and other home-care teaching programs.¹⁶ Here the attempt, broadly speaking, is to provide contact with patients and communities early in the medical school experience by placing the medical student in the capacity of "health advisor," with limited responsibility and careful faculty guidance. Through experience with human beings in their home environments and in the hospital, in principle, the student gains a vivid appreciation of the social and emotional forces in health and illness. In this type of teaching, the sociologist acts as part of a faculty team, advising about problems as they arise in the students' experience. Actually, the educational methodology involved is still highly fluid, and there is active experimentation in the use of different teaching techniques. Nevertheless, the emphasis is on case demonstration in a teaching conference, with principles of behavior fitted in when appropriate.

The second major method of teaching is more didactic, using both lectures and seminars. In this case, fundamental concepts and theories are stressed, following the model of graduate education in academic departments of the liberal arts college. Laboratory work, if employed, is used primarily to demonstrate methods of research.

What is taught combines with how it is taught, as one would expect. Moreover, where the sociologist teaches (that is, in what type of departmental setting) is associated with the combination of content and method.

Public health and preventive medicine have fostered the collaboration of sociologists most

* By Samuel W. Bloom.

¹⁰ Of the 19 sociologists classified as full-time teaching faculty of the medical school, ten are in departments of psychiatry, five in preventive medicine and public health, two in a department of behavioral science in a new medical school, one in a department of medicine, and one is unclassified.

¹¹ This tendency may be reversed or at least "balanced" by an expanded role for sociologists in the growing number of home-care programs, comprehensive clinic programs, family study seminars, and the like, which have become part of medical education in recent years.

¹² Assuming that these are appropriate descriptive categories for the two kinds of approaches to teaching medical sociology, it should be noted that what is taught under these headings is modified quite markedly from what would be taught under them in the conventional undergraduate or graduate class. In the medical school, population-demography material is essentially epidemiologically oriented, while the "social system" material is oriented to the understanding of therapeutic relationships and to institutions like the hospital.

¹³ Odin W. Anderson, "The Sociologist and Medicine: Generalization from a Teaching and Research Experience in a Medical School," *Social Forces*, 31 (October, 1952), pp. 38-42.

¹⁴ L. J. Henderson, "The Patient and Physician as a Social System," *New England Journal of Medicine*, 212 (May, 1935), pp. 819-823; "The Practice of Medicine as Applied Sociology," *Transactions of the Association of American Physicians*, 51 (1936), pp. 8-22; and Pareto's *General Sociology: a Physiologist's Interpretation*, Cambridge: Harvard University Press, 1937.

¹⁵ Talcott Parsons, *The Social System*, Glencoe, Ill.: Free Press, 1951, pp. 420-480.

¹⁶ U. S. Department of Health, Education, and Welfare, *A Study of Selected Home Care Programs*, Public Health Monograph No. 35, edited and issued by Public Health Reports. The Family Health Advisor Program at Pennsylvania was first described in two papers: John P. Hubbard et al., "The Family in the Training of Medical Students," *Journal of Medical Education*, 27 (January, 1952) pp. 10-18; Kenneth E. Appel et al., "Psychiatric Values in a New Method of Medical Education," *American Journal of Psychiatry*, 109 (August, 1952), pp. 102-107.

specifically in the fields of population and demography. In their 1952 detailed report, the Conference of Professors of Preventive Medicine stressed the importance of epidemiology for the study of medicine and extended a warm invitation to the social sciences to join, on an equal footing, the medical school faculty.¹⁷ However, when the preventive medicine teachers experiment with what they have called the "human relations laboratory," which usually involves some contact with patients, they appear to be uncertain about the contribution of the sociologist. Anderson, from his experience as a sociologist in a department of public health, recommends that the social worker should assume most active responsibility for the application of sociological knowledge to the individual patient and to the interactional situation between the doctor and the patient.¹⁸ This opinion seems to be shared by professors of preventive medicine who, in Family Health Advisor programs at both Pennsylvania and Cornell, have relied on social workers rather than sociologists for special collaboration.

Psychiatry, on the other hand, has extended a more unequivocal invitation to sociologists and anthropologists to join in the teaching of a basic science of human behavior in the medical curriculum. A good summary view of this position appears in the final report of the 1951 Conference on Psychiatric Education.¹⁹ This report notes the general tendency of medical education to provide for psychiatric teaching in all years of the medical course. Thus, although traditionally defined as one of the clinical sciences, psychiatry is placed in the unique situation of being the only clinical department which has responsibility for teaching its own basic science. The increase in the amount of responsibility for teaching preclinical courses in psychiatry appears to be directly related to the acceptance of behavioral scientists as teaching colleagues in departments of psychiatry. This type of collaboration was approved by the Ithaca Conference of 1951, the report of which includes a chapter (by Norman Cameron) suggesting an outline for a course entitled "Human Ecology and Personality;" the sociologist is assigned a major role in the teaching of this course, in collaboration with a psychologist and a psychiatrist. More recently, the

Committee on Medical Education of the American Psychiatric Association published an outline of a psychiatric curriculum in which a responsible place is suggested for the social scientist.²⁰ By and large, the orientation of psychiatry in this trend toward a preclinical course in behavioral science has favored the lecture and seminar methods of teaching.

SOME RECOMMENDATIONS ON WHAT TO TEACH *

Turning in more detail to what sociologists should teach in medical education, attention may be directed to the fact that the present trend in this area is away from the extreme segmentation and specialization of the past decades to a more comprehensive and integrated approach. Both medical sociologists and their medical colleagues often hold the opinion that sociology should enhance this integrative process rather than add one more compartmentalized field of knowledge. This point of view is now being implemented in a newly formed department of behavioral science at the University of Kentucky, which has incorporated two major objectives for the department appropriate to medical education. The first is to provide future physicians and other health personnel with a conceptual perspective in terms of which to understand the impact of society and culture on disease and its management. The second is to illustrate the application of this perspective to specific health problems and the life experiences of patients, as well as to the organization of medical services and the experiences of health personnel themselves.

Emphasis on a number of concepts and approaches appears desirable in order to achieve these goals. First, the concept of health and medicine as a behavioral system should prove useful. Sociologists might stress the interrelationships of the following five behavior systems, or systems of human adaptation: (a) internal body system, (b) the environmental system, (c) personality, (d) social system, (e) cultural system. No problem of health can be fully understood unless these systems and their connections are investigated. Second, the sociologist can provide an historical and cross-cultural perspective, enabling the medical student to relate developments in medicine and the organization of medical care to other major areas of human concern such as government, religion, and the arts. Third,

¹⁷ Association of American Medical Colleges, *Preventive Medicine in Medical Schools*, Baltimore: Waverly Press, 1952, pp. 90-91.

¹⁸ Anderson, "The Sociologist and Medicine . . .," *op. cit.*

¹⁹ American Psychiatric Association, *Psychiatry and Medical Education: Report of the 1951 Conference on Psychiatric Education*, Baltimore: Lord Baltimore Press, 1952.

²⁰ The Committee on Medical Education of the American Psychiatric Association, "An Outline for a Curriculum for Teaching Psychiatry in Medical Schools," *Journal of Medical Education*, 31 (February, 1956), pp. 115-120.

* By Robert Straus.

the conception of alternative responses to illness is important. Recourse to medical science is but one of several responses which an individual may make when faced with problems of ill health; he may, for example, seek help from pseudo-medical sciences, folk medicine, or faith medicine. The medical student should realize that these responses are not restricted to certain historical periods or remote places, but are also found in our present society. Finally, the behavioral sciences can contribute significantly to an understanding of such major life experiences in the human life cycle as pregnancy, childbirth, infancy, and death. The physician's experiences will be more complete and effective when he can relate his knowledge of biological process to pertinent social, cultural, and personality factors.

THE ROLE OF THE SOCIOLOGIST:
A PHYSICIAN'S VIEW *

The most appropriate location for the sociologist in the medical school depends on the particular social structure of the school in which he finds himself, and on how he defines his teaching and research aims. Both will necessarily vary to some degree from school to school and from sociologist to sociologist. Medical schools tend to be somewhat more uniform than are sociologists, but it is nevertheless possible to consider, in general terms, how the sociologist can best fit himself into the medical school environment so that he is both satisfied and effective.

Almost all American medical schools are composed of a dozen or more autonomous departments, about equally divided into those dealing with basic science subjects and those concerned with clinical teaching. Some departments, such as public health and psychiatry, may include both orientations and, therefore, they bridge the conceptual gap between the first and last two years of the curriculum. The dean, although the most important administrative figure, usually functions as a moderator of the committee of department heads who formulate educational policy for the school. He rarely accomplishes his ends directly, but works through one or more of the departments, or through the committee of department heads.

In certain medical schools, for example, Western Reserve, Harvard, and Cornell, there may be an integrated curriculum that allows several departments to join in some aspect of the teaching enterprise. As the education of the medical student tends more and more to be considered as a whole, it may be expected that such integrated teaching will become more widespread, and therefore perhaps will allow the so-

ciologist, as well as other teachers, greater scope and flexibility.

The kind of work the sociologist wants to carry out in the medical school is an important consideration. Does he think of his role as that of teaching basic knowledge about human social behavior? Or does he feel that he can contribute most by applying this knowledge to the understanding of specific practical problems, for example, those involved in the management of patients or the organization of medical services for a community? Does he prefer the lecture method, the small group conference, or membership in the clinical team? What are his research interests and what is his training? Is he a demographer, concerned with social structure, or perhaps with small-group processes? Whatever his orientation and preferences, like other academic persons, he probably hopes for an adequate salary, research facilities, some autonomy in his work, and a chance for tenure and promotion.

The experiences of those who have thus far pioneered in this new field have been varied, as others have indicated above. Some sociologists have taken positions in departments of public health, others in psychiatry. There have been a few in departments of medicine, and some have been attached to integrated teaching programs. A number of deans, enthusiastic about the "new" discipline, have brought sociologists into medical schools under their own auspices, without departmental affiliation. Many have had primarily a research role, and very few have limited their activities entirely to teaching; occasional rather than regular lectures appears to be the more frequent pattern.

With these considerations in mind, it is possible to make some tentative statements about what may be, in the long run, the most satisfactory way for the sociologists to become integrated into the medical school. Faculty and students alike often consider departments of public health and psychiatry somewhat peripheral to the main business of the medical school. Affiliation with either of these departments, therefore, may cause the sociologist to inherit the problems of legitimating the contribution of these departments, in addition to those entailed in establishing his own discipline. Nevertheless, either public health or psychiatry may afford him an appropriate position in the faculty structure. An appointment in psychiatry sometimes carries the additional risk that the sociologist may be subordinated to the psychiatrists, who are likely to believe that they should have the last word on all aspects of human behavior. This, at least, has frequently been the fate of psychologists who have worked in departments of psychiatry.

* By George G. Reader, M.D.

Being sponsored by the dean, on the other hand, may have real advantages—if the dean is able to create departmental status for the sociologist, as at the University of Kentucky. Without such status, the sociologist may face important difficulties in obtaining access to teaching time or even research space.

The best location for the sociologist may be a department of medicine, even though here he will inevitably be under continuing pressure to demonstrate the relevance of his discipline in the actual care of patients. If through scientific investigation, however, he does contribute to the body of knowledge that constitutes clinical medicine, his place in the medical school is probably assured. He takes his place most successfully as a scientist, and in this way gains prestige and acceptance. Gradually his views may become more and more influential in the work of medical education, as his findings become more and more important to the proper care of patients. In fact, his teaching role may be pursued successfully in any department of the medical school so long as his research contribution to the medical setting is sound.

The value system of the medical school, as it relates to both teaching and academic status, places scientific endeavor and optimal patient care in first place. To be accepted in the medical school the sociologist must therefore acknowledge these values and adapt himself accordingly.

DISCUSSION *

Obviously, no single answer to the question of what and how to teach sociology in medical education is possible. There is sufficient difference among medical schools themselves so that the development of the role of the sociologist in medical education has followed different patterns, dependent upon the particular institutional settings. Nevertheless, some strongly partisan feelings exist among both sociologists and their medical colleagues concerning this matter.

On the one hand, there is the opinion that the sociologist should engage in research on problems of patient care, to be followed by his gradual incorporation into the professional group conducting clinical medical care. Perhaps the clearest example of this approach is to be found at Cornell in the Comprehensive Care and Teaching Program; a similar point of view is evident in the curriculum revision at Western Reserve.²¹

* By Jerome K. Myers and Samuel W. Bloom.

²¹ See "Reports on Experiments in Medical Education," by Joseph T. Wearn, T. Hale Ham, John W. Patterson, and John L. Caughey, Jr.; by Fred Kern, Jr., and Kenneth R. Hammond; and by

On the other hand, the increasing number of full-time appointments in departments of psychiatry and preventive medicine, which emphasize preclinical instruction and basic research, places the sociologist in a position analogous to the other so-called basic scientists in medicine.

Related to differences of the sociologist's formal position in the medical school are the questions of *how* he can teach more effectively. Stated most succinctly, the controversy appears to be between the case conference and the lecture-seminar method.

Another controversy may be reflected by the fact that sometimes the sociologist is viewed primarily as a faculty colleague, sometimes as a skilled consultant.

How, then, can the present position of the sociologist as a medical educator be summarized? The answer would seem to be that he is placed in the highly unstable but exciting position of an innovator. He is privileged, in comparison with colleagues in the liberal arts college, to have less burdensome teaching duties, more financial support, and encouragement for research. He is burdened, on the other hand, with a measure of isolation and loneliness that seems to be a part of ploughing new fields.

His most pressing problem probably is what Stainbrook calls "the problem of relevance."²² This is the question of what concepts, theories, and methods in sociology are most directly relevant for medical education, and how can they best be communicated to medical students. There is sufficient variation in where the sociologist is placed in the social structure of the medical faculty to allow the examination of what differences, if any, the departmental setting makes in his teaching effectiveness. This problem of relevance commands his most intensive effort. In support of this effort, it may be recommended that maximum communication be maintained among sociologists concerning the content and methods of their teachings and, in general, the nature of their experience in medical education. Toward this end, both new policies of publication in the *American Sociological Review* and the expansion of the forum provided for medical sociology by the American Sociological [Association] at its annual meetings are encouraging.

George G. Reader, *Journal of Medical Education*, 31 (August, 1956) pp. 516-552; see also Reader, "The Cornell Comprehensive Care and Teaching Program," in *The Student Physician*, *op. cit.*, pp. 81-104.

²² Edward Stainbrook and Murray Wexler, "The Place of the Behavioral Sciences in the Medical School," *Psychiatry*, 19 (August, 1956), pp. 264-269.

COURSES PREFERRED FOR ADMISSION TO GRADUATE DEPARTMENTS OF SOCIOLOGY *

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In reviewing candidates for admission to graduate study in sociology, departments use criteria which seem to vary a good deal. Especially for departments which have undergraduate work only, this diversity of expectations makes the advising of potential graduate students difficult. The student does not know which departments are going to accept him or grant the necessary financial inducements.

Moreover, the undergraduate department is perplexed when it faces painful decisions about its necessarily limited curriculum. Preparation for graduate study is only one of its functions, but a crucial one. Are there certain courses that must be taught for this purpose, no matter which graduate department the student enters? Are some new expectations emerging of which undergraduate departments should be made aware?

These questions suggest others: Are there some logical sequences of sociological materials that could be planned more effectively? To what extent might consensus on sequential arrangements of courses be advantageous even where there is no pyramiding of material? Is sociology mature enough for widespread agreement on these matters? Would more attention to this problem improve the efficiency of graduate as well as undergraduate instruction?

To throw some light on these questions, a questionnaire¹ was mailed during the 1958-59 school year to the chairmen of all the 114 departments of sociology in the United States known or believed to have programs of graduate study.² Replies were received from 99 departments.³ Six of these indicated that graduate work had been discontinued; one noted that it is

nominal. These seven returned uncompleted questionnaires, and it is very likely that at least five of the non-responding departments also have no graduate study. Thus, this report is based on 92 completed questionnaires, about nine-tenths of the estimated number of ongoing programs of graduate study in sociology.

Forty-nine of the 92 chairmen reported that they have doctoral programs. During 1955, 1956, and 1957, seven of these departments granted no doctorates; 21 conferred from one to five; six gave from six to ten. Hereafter, those seven which granted from 11 to 15 degrees during this three-year period will be referred to as "middle-sized doctoral programs"⁴ and those eight which conferred 16 or more will be called "large doctoral programs."⁵ Presumably these figures include doctorates in anthropology in the case of combined departments. The size of the M.A. programs is only moderately correlated with the size of the doctoral program.⁶

Only two-fifths of the chairmen indicated that an undergraduate major in sociology is required for admission to graduate study. The proportion requiring it was the same for doctoral and for Master's-only programs. Some commented that the sociology major is preferred but not required. The general position of the majority is illustrated by the following statement by the chairman of one of the large doctoral departments:⁷

We have no fixed pattern for admission other than that the student have a 2.7 average ($B = 3.0$), and that he have 40 quarter hours in sociology and related fields. We would prefer that undergraduate training be broad in character rather than concentrated in sociology. Preferably this should include some training in college mathematics and statistics, as well as in related social sciences.

Of the 89 who replied to an item concerning the Graduate Record Examination, 18 checked "Required," 33 "Encouraged," and 38 "Not-used." Doctoral programs require it slightly more often than do those for the Master's only,

* Paper read at the annual meeting of the American Sociological Association, Chicago, September 1959. Student assistants David Roberts and David Hollister deserve thanks for careful work at various stages of the project.

¹ The pre-test draft went to 15 chairmen and other sociologists in different regions. Revisions were based on checks for ambiguity and volunteered suggestions.

² The questionnaire went to all departments in the 1956 list made available to us by the officers of the American Sociological Society (Association), plus certain ones omitted from that list.

³ The first mailing brought 70 returns, the remainder came after a follow-up (again with stamped return envelopes).

⁴ These universities are: New York University, Northwestern, Minnesota, Yale, Southern California, Duke, and Iowa (Iowa City).

⁵ Columbia granted the most (36). The other universities are: Cornell (28), Chicago (22), Michigan (40), Iowa, Iowa City, (34), Columbia (33), (18), North Carolina (16), and Ohio State (16). The only non-respondent that would probably be in this group is Harvard.

⁶ The largest M.A. programs in terms of degrees granted during the three-year period are: Michigan (40), Iowa, Iowa City, (34), Columbia (33), N.Y.U. (30), North Carolina (30), Chicago (26), and California (23). Next come Illinois, Wisconsin, Southern California, Yale, and Washington (St. Louis).

⁷ Ohio State University.

but the corresponding responses for the large doctoral departments were one, four, and three; for the medium-sized doctoral departments, two, four, and one.

Most of the questionnaire is devoted to a list of 41 courses in sociology and 11 in allied fields. These were placed in small homogeneous groups, but no headings were used to describe the groups. No attempt was made to be exhaustive; only courses which are widely taught and which might be considered essential preparation for graduate study were included. Course names which would be widely understood by sociologists were utilized, and some revisions were made after the pre-test of the instrument. The chairmen were asked to consider each item to be a separate course offering. They were also asked to rate each course by the use of the following code:⁸

1. Required for admission to graduate study, and must be taken for credit if student has not had it.
2. Absolutely essential for graduate study, but may be taken for graduate credit if student has not had it.
3. Desirable for beginning graduate students to have taken, but not absolutely essential.
4. Acceptable as part of the preparation for graduate study.
5. Not desirable as part of the preparation for graduate study.

Category "1" was supposed to have read, "for no credit." Few qualified their "1" ratings in the margin, but in open-end comment 21 chairmen said they do require some work to be taken without credit. Ten did not specify the course or courses. Of the eleven who did, six indicated Elementary Statistics, one Advanced Statistics, one Development of Social Thought, and two Introductory Sociology. One-fifth or more of our graduate departments, then, require some work (usually statistics) for no credit if it has not been taken. This means that many of the "1" ratings received would still have been "1"s if the word "no" had been included, but perhaps many would have been "2"s. The "1"s must be interpreted as little different from the "2"s, and perhaps some of the higher mean scores are higher than they would have been without the error.

The main findings of the study appear in Table 1, which shows the mean ratings of the courses in each group in descending order of judged importance. Most of the groups represent convenient, but not entirely satisfactory, combinations of smaller groupings of courses in the questionnaire. The scores in the several tables

TABLE 1. MEAN RATINGS BY GRADUATE DEPARTMENTS IN 1958-59 OF SELECTED COURSES AS PREPARATION FOR GRADUATE STUDY IN SOCIOLOGY

	Mean Rating
Courses in General Sociology and Theory	
Introduction to Sociology	1.7
Principles of Sociology (not the Introductory course)	2.5
Contemporary Sociological Theory	2.5
Development of Sociological Theory	2.6
History of Social Thought	2.7
American Society	3.5
Courses in Social Problems	
Social Problems or Social Disorganization (Freshman-Sophomore level)	3.1
Social Pathology or Social Disorganization (Junior-Senior level)	3.3
Social Change	3.3
Racial and Cultural Minorities	3.3
Criminology	3.3
Juvenile Delinquency	3.3
Marriage	3.5
The American Negro	3.6
Courses in Social Psychology	
Introduction to Social Psychology	2.8
Collective Behavior	3.2
Culture and Personality	3.3
Social Control	3.4
Small Group Interaction	3.4
Public Opinion and Propaganda	3.6
Courses in Human Ecology and Demography	
The Community	3.0
Population Analysis	3.1
Urban Sociology	3.2
Rural Sociology	3.3
Human Ecology	3.3
Courses in Methods of Social Research and Arrangements for Individualized Study and Research	
Elementary Statistics	2.0
Methods of Social Research	2.1
Advanced Statistics	3.0
Independent Study (supervised individual library or empirical research)	3.1
Seminar (discussion of common readings)	3.2
Seminar (discussion of individual library research projects)	3.2
Seminar (discussion of individual empirical research projects)	3.3
Courses in Social Organization	
The Family	3.0
Social Organization	3.1
Social Institutions	3.2
Social Stratification	3.3
Industrial Sociology	3.5
Political Sociology	3.7
Sociology of Religion	3.7
Medical Sociology	3.9
Sociology of Education	4.0

⁸ Spaces were left at the end of each of the course groupings for the addition of other related courses if the respondent would rate them "3" or higher, the precise rating to be indicated.

TABLE 1—Continued

Selected Allied Subjects	
Cultural Anthropology	2.8
Introduction to Psychology	3.0
First-Year College Mathematics	3.1
Principles of Economics	3.4
Elements of Political Science	3.4
Introduction to Philosophy	3.4
Calculus	3.6
General Biology	3.7
Survey of Social Work	4.1
Group Work (classwork with field experience)	4.1
Case Work (classwork with agency experience)	4.1

range from 1.7 to 4.1, which means that no course received a mean placement in the extreme end categories of the continuum. Thus 2.5 and 3.5 become the important cutting points. A mean rating above (smaller than) 2.5 indicates that the average chairman considered the course to be "absolutely essential" for graduate study, although it may be taken for graduate credit. A mean rating between 2.5 and 3.5 signifies that chairmen tend to consider the course "desirable but not absolutely essential." A mean rating below (greater than) 3.5 represents the judgment that the course is "acceptable" as preparation for graduate study. Mean ratings of exactly 2.5 and 3.5 are interpreted as indicating equal amounts of preference for the categories on either side.

In these terms, the table shows that the introductory course is the only course listed in *General Sociology and Theory* which received a clear mandate as "absolutely essential" for graduate study.⁹ Opinion was equally divided as to whether Principles of Sociology and Contemporary Sociological Theory are "absolutely essential" or "desirable but not essential," while the average vote inclined towards "desirable, etc." for Development of Sociological Theory and History of Social Thought. American Society is rated half-way between "desirable" and "acceptable."

The table shows that most of the general and specific courses in *Social Problems* were judged to be "desirable but not absolutely essential" for graduate study. Most of the chairmen used "3"s and "4"s in rating these. The American Negro falls on the side of "acceptable" rather than "desirable," and Marriage lies half-way between.

⁹ Two-thirds rated it "1"; there were a few "2" and "4" ratings, and eleven "3"s. About three-fourths replied that the introductory course is offered on a one-semester or one-quarter basis. Forty-four give three semester-hours credit; 22 give six semester-hours; 14 give five quarter-hours; all other amounts of credit are infrequent.

It is likely that these two courses received fairly similar ratings for quite different reasons: the former may be viewed as too specialized for undergraduate study, and Marriage—as a functional or applied course for the general student—not sociological enough.

Although it received a rating of "1" or "2" from about one-fifth of the departments, and was conceded more importance than other courses in its group, Introduction to Social Psychology¹⁰ emerged as "desirable but not absolutely essential" for graduate study. Other courses in the group received the same mean placement, shading off to Public Opinion and Propaganda, which falls on the "acceptable" rather than the "desirable" side. All the listed courses in *Human Ecology and Demography* were classed as "desirable but not essential," with a good many "4" ratings pulling the means down from the modal "3"s.

In the fifth group we find two "absolutely essential" courses, Elementary Statistics and Methods of Social Research. Perhaps many will be surprised to see these two courses rated as of almost equal importance. No longer is one "tool course" sufficient; the consensus must be that beginning graduate students need to know more about research processes than the rudiments of statistics.¹¹ Departments offering the M.A. only gave a higher mean rating (1.8) to elementary statistics than those which offer the Ph.D. (2.3). Advanced Statistics and various arrangements for individualized study and research are all rated "desirable but not essential." Evidently some seminar experience is thought desirable before graduate study begins, although not necessary.

Half the listed courses in *Social Organization* fall on the "desirable but not essential" side of the cutting point, half on the "acceptable" side,

¹⁰ Thirty reported that they give this course on the Freshman-Sophomore level, 46 the Junior-Senior level, and some of the other 16 indicated that this is a graduate-level course. The large doctoral departments rated this course higher (2.5) than did the medium-sized ones (3.1).

¹¹ A recent study shows that from 1942 to 1957 there was a considerable decline in the number of research methods courses. See L. Podell, M. Vogel-fanger, and R. Rogers, "Sociology in American Colleges: Fifteen Years Later," *American Sociological Review* 24 (February, 1959), pp. 87-95. It is also reported that undergraduate departments give a higher proportion of value-laden courses than graduate departments do, but this conclusion is debatable. Certain course areas, such as crime, inter-group relations, and industrial relations, were treated as if they are value-laden by definition, while sociology of religion, educational sociology, social movements, and others, are not. Surely this is oversimplified. Does science inhere in the method or the subject matter? Isn't the proper question, "How are courses taught?"

and Industrial Sociology half-way between. The Family rates highest in this group, and it may surprise some teachers to learn that it is not considered essential for graduate study. Perhaps the reason most of these courses in *Social Organization* are not rated "essential" is that they represent areas of heavy interest on the graduate level, particularly Social Stratification and those that follow in the table. The same point probably holds for much of advanced social psychology, ecology, and demography, and for specialized problem areas.

Cultural Anthropology tops the list of allied subjects, possibly because there are now a good many combined sociology-anthropology departments.¹² It is rated "desirable but not absolutely essential," just a little above Introduction to Psychology and First-Year College Mathematics. Principles of Economics barely makes the rating of "desirable," along with the beginning courses in political science and philosophy. While generally judged acceptable, the three courses listed in social work received the lowest rating of all the courses on the questionnaire as preparation for graduate work in sociology.¹³

What may be concluded on the basis of these returns? The only courses the graduate departments generally rated "absolutely essential" as preparation for advanced study are Introduction to Sociology, Elementary Statistics, and Methods of Social Research. It would seem reasonable, on the basis of this judgment, to require these courses for admission and to insist that otherwise they must be taken in graduate school for no credit. Apparently most graduate departments allow credit for these courses, although several indicate that they do not. Views on undergraduate preparation and admissions policies evidently are quite varied at present. Few differences were found between graduate departments of different size and character; there are great variations within each size and type.

Principles of Sociology and Contemporary Sociological Theory, on the average, fall half-way between the "absolutely essential" rating and "desirable but not absolutely essential." Most of the remaining courses listed were considered "desirable," the highest ratings in this large group going to Development of Sociological Theory, History of Social Thought, Introduction to Social Psychology, and Cultural Anthropology.

These findings indicate that what graduate departments most want their students to have

studied are good introductory syntheses of the major phases of sociological effort.¹⁴ Research frontiers are ever-shifting, and material keeps filtering down from graduate seminars to undergraduate courses. This limited exploration suggests the conviction of graduate departments that the "trickle-down" process should occur in relatively general courses rather than in highly specialized ones—for example, that undergraduate departments should teach about small groups in Introduction to Social Psychology.

Perhaps it might be agreed that at least some of the "desirable" group, in addition to those judged "absolutely essential," are the responsibility of undergraduate departments. Illustrative courses are Social Psychology, Social Problems, The Family, Collective Behavior, The Community, Social Institutions, and History of Social Thought.

It may also be the consensus that courses rated "acceptable" rather than "desirable," are properly the province of the graduate departments and should not be taught at the undergraduate level. These are courses in such specialized areas as the American Negro, Political Sociology, Sociology of Religion, Medical Sociology, and Sociology of Education. Views concerning such courses as Criminology, Social Control, Culture and Personality, Human Ecology, Social Stratification, and Industrial Sociology are problematic. Further study of this problem, cast explicitly in terms of the possibility of a more clearly defined division of labor between undergraduate and graduate departments, might clarify some of the issues raised here.¹⁵

THE CONTEMPORARY POLISH SOCIOLOGICAL REVIEW

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The most comprehensive review of modern Polish sociology written in English is probably

¹⁴ The same type of course, with the possible exception of some work in problem areas, is also the most useful for the other goals of liberal arts (and teacher) education. The only courses that must be added for the sociology major concern methods of research, the development of theory, and possibly those in such socially important areas as family and community.

¹⁵ In a paper by James W. Wiggins, "The Undergraduate Curriculum in Sociology and Graduate Education in Sociology," presented at the 1955 meeting of the Southern Sociological Society, it was urged that undergraduate departments be encouraged to offer general sociology, elementary statistics, research methods, and anthropology.

¹² No check was made for this. Ratings of Cultural Anthropology ranged from "1" to "5"; "3" was the mode; there were nine "1"s and 16 "2"s.

¹³ There was an average (mean) of 20 "5" ratings for each of these three courses.

Eileen Markley Znaniecki's paper, published in *Twentieth Century Sociology*.¹ A later summary is Abel's 1950 article in the *American Sociological Review*.² The most recent survey of Polish sociological endeavors is in French, in *Cahiers Internationaux de Sociologie*.³ The period from 1918 to 1947 in Polish sociology was summarized, in Polish, in a 1948 article.⁴

The present note adds to our meager knowledge of Polish sociology by summarizing and discussing briefly the 1957 and 1958 volumes of the *Polish Sociological Review* (*Przegląd Socjologiczny*). This journal reappeared in 1957, after a lapse of nine years, and is, to the writer's knowledge, the only sociological periodical published in a communist country today.⁵ Its chief editor, Jozef Chalasiński,⁶ was a student of Znaniecki.

The journal is unusual in that each year's volume is published all at one time, like Durkheim's *L'Année Sociologique*. Each volume runs approximately 450 pages and carries some articles as long as 50 pages. While the 1957 volume carried no reviews or news, the 1958 volume devoted more than a quarter of its pages to this kind of content.

The year 1957, in which *Przegląd Socjologiczny* reappeared, can be regarded as a small renaissance in Polish sociology, in spite of the remaining limitations upon empirical investigation in Poland. The 1957 volume contains papers by ten authors but only one paper, by W. Piotrkowski, is based upon empirical research. This paper describes the careers of students who attended special preparatory courses and later pursued technical college curricula.

These young men and women were recruited mostly from farms and the working class, so that their careers symbolize the considerably changed social structure. "Individual inquiry cards"⁷ were used as a principal data-gathering instrument in the study, following the traditional "Polish method of biography."

On the whole, the 1957 volume is historical and discursive rather than sociological. The discursive articles mostly interpret ideas of other authors, quoting them frequently. Three of the ten articles are concerned with problems of education.⁸ Frequent references to Lenin appear in several papers. On the other hand, the authors show a certain independence in criticizing other writers living in communist countries.

The 1958 volume, containing nine papers plus reviews and news, is predominantly sociological, although several articles may be regarded as more historical than sociological. Three of the papers are based on actual Polish research. The papers give little information about the methodology used, and in none is there any indication that statistical analysis was employed. Even so, this volume represents considerable progress as compared with the 1957 volume.

The 1958 volume opens with a survey article on small groups by A. Kłosowska, in which she relates small group studies to small community investigations. She covers most of the major American contributors, with one conspicuous omission—no reference is made to Bales. She pleads for the development of small group studies in Poland, and stresses the need for "piecemeal social engineering." She feels that small group study should be emphasized at the present time, especially since the major changes in economic-political structure in Poland presumably have already been carried out.

St. Nowakowski offers interesting material in the 1958 volume on workers living in so-called workers' hotels. His paper reports research, undertaken in Warsaw, on a hotel housing about

¹ "Polish Sociology," in Georges Gurvitch and Wilbert E. Moore, editors, *Twentieth Century Sociology*, New York: The Philosophical Library, 1945, pp. 703-717.

² Theodore Abel, "Sociology in Postwar Poland," *American Sociological Review*, 15 (February, 1950), pp. 104-106.

³ C. Andrieux, "La Sociologie Polonaise," *Cahiers Internationaux de Sociologie*, 24 (1958), pp. 167-175.

⁴ Jozef Chalasiński, "Trzydziest lat socjologii polskiej, 1918 do 1947," *Przegląd Socjologiczny*, 10 (1948), pp. 1-54.

⁵ In Yugoslavia, a semi-sociological periodical, *Filozofija i Sociologija*, has appeared recently. See *Kölnner Zeitschrift für Soziologie und Sozialpsychologie*, 10 (1958), pp. 322-328.

⁶ Chalasiński is also chief editor of another cultural-sociological periodical, *Kultura i Społeczeństwo* (*Culture and Society*), a quarterly concerned especially with the analysis and criticism of non-material Polish culture.

⁷ The use of the individual inquiry card, as applied to industrial personnel, has been described in a UNESCO bulletin dedicated to Poland. See J. Malanowski, A. Sarapata, and S. Szostkiewicz, "The Individual Inquiry Card as an Instrument of Research into the Structure of the Personnel of a Work Establishment," in "Social Change in Poland," *International Social Science Bulletin*, 9 (2, 1957), pp. 212-225.

⁸ The emphasis upon education in present Polish society is also stressed by J. Szczepański in "Poland," in A. M. Rose, editor, *The Institutions of Advanced Societies*, Minneapolis: University of Minnesota Press, 1958, p. 268.

3,000 workers engaged mainly in construction. About 100 interviews were obtained. The author explains that demoralization among these workers was caused partly by losing contact with their families left behind in the rural areas. This reminds one of the Thomas-Znaniecki thesis: that so long as the worker keeps alive his contacts with the family in the country, he is still a peasant in the city. The change from peasant to worker is a critical transition, which, if not accompanied by the acquisition of new ties and interests, can result in personal disorganization. Several of the persons interviewed did succeed in developing new educational and aspirational goals. The author concludes that there is a mass phenomenon of urbanization of the rural population in Poland.

Z. T. Wierzbicki, in the same volume, presents a community study of a Polish village. His major problem was the motivation behind the decision whether to send the child to higher schools or to keep him at home. He finds that farmers and their children feel that intellectual work is "lighter." The motivation was frequently explained in negative terms—the desire to escape the hard work of rural life.

Finally, H. Radomska-Strzemecka presents data from an unpublished study of Polish teenagers during World War II. During 1945–49, interviews were obtained in several Polish cities from 6,205 Polish boys and girls. The article analyzes relationships between parents and children. Among the children, 57 per cent felt that attachment to their parents had increased during the emergency, while 36 per cent considered it unchanged. In only a small minority of cases was there a lessening of cohesion within the family. On the whole, it appeared that the family had resumed several functions assumed by other institutions in normal times, thus developing stronger ties. Only in Jewish families, where the wartime danger was greater, did the family, failing to withstand such pressure, break up. This study appears to warrant publication in its entirety because of the interest in such material to students of comparative family systems and problems.

News items contained in the 1958 volume include the following: the Polish Institute of Sociology and History of Culture, a section of the Polish Academy of Science, is analyzing contemporary trends in American and Western sociology; the Lodz Section of the Institute is conducting several studies of college graduates and their careers; the Warsaw group has undertaken several studies of industrial workers; and the Catholic University of Lublin's Institute of

Sociology is engaged in a study of juvenile delinquency.

Of the books reviewed, eight were published in France, six in Poland, three in Germany, two in the United States, and one each in England and the Netherlands. Most of these books are in the field of industrial sociology. Sociological periodicals also are reviewed: for example, issues of the *American Sociological Review*, the *American Journal of Sociology*, and *Cahiers Internationaux de Sociologie*. A survey article, by J. Kadzielski, reviews major Soviet theses on culture as published in *Voprosy Istorii* and *Voprosy Filosofii*.

ON "TEXTBOOKS": A COMMUNICATION

LOUIS M. HACKER

Pennsylvania State University

It is high time that learned societies and professional associations of scholars took cognizance of a growing and, I should say, evil practice on the part of college-text publishers of making up books out of other people's writings. These are so-called "books of readings" or "introductions" to this or that discipline or combinations of disciplines; they are clip and paste jobs with sometimes a pat on the head of two or three lines for the original author and sometimes no more than a cavalier nod in his direction in the form of a footnote.

In any case, this is easy money for the compiler and the publisher and a poor way (as regards scholarship and teaching) of getting a book together.

But what I wish to protest against is that apparently the practice is developing among publishers of not even asking the authors for their permission to appear in these collections, some of which are very sorry performances indeed; they simply ask their fellow-publishers (despite the fact that usually copyrights are held by authors) who, because they are in the same cheap game themselves, readily give their consent. The defenseless author does not know he is being used until he comes across the work by accident. Needless to say, in this kind of flim-flam, no one bothers to talk of compensation for reprinting.

Time was when compensation was the ordinary and honorable procedure with permission first obtained from the author before the publisher (simply as a matter of courtesy) was approached.

I recently had an outrageous experience in which I found myself reprinted three times in

a melange which purported to be an introduction to the social sciences. This was done without my knowledge, consent, or, of course, any compensation. In fact, the publisher resented my asking for three free copies of the book and, after an exchange of a number of letters, ended up grudgingly by sending me one, and that one a paperback.

I am calling upon learned societies to protect their members against this sort of high-handed treatment and to formulate rules for permissions

and compensation.* It is all very well—and proper—to protest against the Russian piracy of American books; but piracy at home is equally reprehensible.

*The Committee on Publications of the American Sociological Association recently established new policy governing the granting of permissions to reprint material from the *Review* and *Sociometry*. The situation discussed by Professor Hacker is now being reviewed by the Association.—*The Editor*.

NEWS AND ANNOUNCEMENTS

SCHEDULE OF ANNUAL MEETINGS OF REGIONAL SOCIOLOGICAL SOCIETIES, 1960

District of Columbia, Washington, D. C.; May 7 (also meets monthly); President Robert T. Bower, Bureau of Social Science Research, Washington 8, D. C.

Eastern, Boston, Massachusetts; April 23-24; President William J. Goode, Columbia University, New York 27, New York (see p. 111 for details).

Midwest, St. Louis, Missouri; April 21-23; President Paul J. Campisi, Southern Illinois University, Carbondale, Illinois.

Ohio Valley, Bloomington, Indiana; April 22-23; President Louis Schneider, Purdue University, Lafayette, Indiana.

Pacific, Spokane, Washington; April 28-30; President Donald R. Cressey, University of California, Los Angeles 24, California. August B. Hollingshead will give the MacIver Lecture on "Some Basic Issues in the Epidemiology of Mental Illness."

Southern, Atlanta, Georgia; April 7-9; President E. William Noland, University of North Carolina, Chapel Hill, North Carolina.

Southwestern, Dallas, Texas; April 15-16; President Warren Breed, Tulane University, New Orleans, Louisiana.

WILLIAM KIRK 1880-1959

William Kirk died July 27, 1959, after an illness of some weeks. His was a long, active, and full life. A native of Baltimore, he turned to Johns Hopkins University for his education, receiving his B.A. in 1902 and his Ph.D. in 1905. He then went to Brown University as an Instructor in Economics, where by 1910 he was Associate Professor of Social and Political Science. In 1911 he moved to the

University of Rochester as Professor and Head of the Department of Economics and Sociology, where he remained until 1920. In 1921 he was called to the University of California at Berkeley as Associate Professor of Social Economics, but he left after one year to accept the chairmanship of the Department of Sociology at Pomona College, where he taught until his retirement in 1946. Thereafter, he continued to teach for some years in the Claremont Graduate School.

While primarily a teacher, devoted to his class work, William Kirk's abilities and services were by no means limited to the classroom. His life-long interest in social welfare led him to give generously of his time and talents in that field. For five years (1911-1916) he was General Secretary of United Charities in Rochester, and in 1919 he was Educational Director of the American Red Cross for Western New York. Even during his one year at the University of California he was made President of the Berkeley Commission of Public Charities. In Claremont he played a significant role in community and church activities. He never hesitated to put theory to the test by its practical application.

He traveled widely in Europe, but it was his consuming interest in primitive peoples that took him all over the world. In eight trips between 1928 and 1947 he sought out surviving preliterate cultures (as well as those literate but retarded) in several countries of the Far East, and in New Zealand and Australia, Central and South America, and Africa. He brought back to his classes the added sociological and anthropological insight thus acquired.

Professor Kirk was a member of Phi Beta

Kappa and several other honorary scholastic societies. He belonged to quite a number of professional scholarly societies—sociological, economic, geographical, and others—for his interests were broad. He helped found the Pacific Sociological Society and was its second president. He served on the Executive Committee of the Social Science Research Conference of the Pacific Coast and later as vice-president. He was author of *National Labor Federations* and editor of *A Modern City*, a study of Providence, Rhode Island. In his later years he confined his writing to research articles, most of them on primitive cultures.

A man transcends his work, however. William Kirk's qualities as a man endeared him to his students and colleagues. He was deeply interested in his students and followed their careers with keen interest. When they returned to the campus years after graduation they looked him up, whether in office or home. As a colleague he was the easiest of men to work with—courteous, kindly, generous. He will be missed, both as a scholar and a man.

RAY E. BABER

Pomona College

ROBERT HORNIMAN DANN

1889–1959

Robert Horniman Dann, Professor Emeritus of Sociology at Oregon State College, died on August 18, 1959. Born in Brighton, England, he left for Canada at the age of 16 and in 1913 he came to the United States. He received a B.A. degree from Pacific College (now George Fox College), Newberg, Oregon and an M.A. from Haverford College in 1918. Further graduate work in sociology took him to Harvard University, the University of Pennsylvania, and the University of Washington. His teaching career began at Guilford College, North Carolina, where he was Professor of Religious Education and Sociology from 1920 to 1924. In 1927, after a brief period in business, he joined the staff of Oregon State College as Instructor in Economics and Sociology, later moving up through the ranks and retiring in 1956 as Professor of Sociology.

Following his retirement, Professor Dann accepted a position in Honolulu as repre-

sentative of the American Friends Service Committee. From 1957 to 1959 he was Professor of Sociology at The College of the Pacific, and he had only recently returned to his home in Corvallis at the time of his death.

Professor Dann combined Quaker convictions with training in sociology to aid his students in the development of a rational approach for dealing with the problems of their communities. His sincerity as well as his unusually large capacity for friendship won for him the high esteem in which students and colleagues held him.

He was seriously concerned with the question of capital punishment, and gathered extensive data from a number of states to test the hypothesis that capital punishment is not a deterrent of crime. His account of "Capital Punishment in Oregon" was published in the *Annals of the American Academy of Political and Social Science*. From 1946 to 1952 he served on the board of the Oregon Prison Association.

During the year 1946–1947 he was the official representative of the American Friends Service Committee to the world assembly of Quakers in Australia. In addition to his very active participation in the programs of the Committee, he was a member of long standing of the Pacific Sociological Society, the American Sociological Society, the American Academy of Political and Social Science, and the American Association of University Professors.

Survivors include his widow, Lyra Miles Dann, a daughter and two grandsons.

HANS H. PLAMBECK

Oregon State College

HARRY JOSEPH WALKER

1905–1959

Harry Joseph Walker, Associate Professor of Sociology at Howard University, died in Washington, D. C., on May 23, 1959. He had been ill for only a few days, and his death came as a shock to his family, colleagues, students, and friends.

Harry Walker was born in Muncie, Pennsylvania, but received his early education in the nearby town of Milton. He began his college work at Bucknell University, but

transferred after his freshman year to Oberlin College, from which he received a baccalaureate degree in 1928, with a double major in sociology and political science. He received graduate degrees in sociology from Fisk University (M.A., 1936) and the University of Chicago (Ph.D., 1945). His graduate study was supported in large part by three fellowships from the Julius Rosenwald Fund. In 1949, Professor Walker was married to LaVergne Johnson; she and Harry Walker, Jr., survive.

It was Walker's ambition to enter law school following his undergraduate work but, owing to the need of financing his own education, he accepted a teaching position in the social studies at Leland College, Baker, Louisiana. The period at Leland introduced him to the southern community and the racial pattern of the South, and it was during this time that he developed an interest in the study of race relations. At the end of two years at Leland, Walker enrolled as a graduate student in sociology at Fisk University, where a program of research centered around race relations in the South was having a promising beginning. He remained at Fisk as student, research assistant, and teacher until 1939, participating in numerous studies on aspects of southern community life and coming under the influence of Charles S. Johnson, E. Franklin Frazier, and Robert E. Park. The influence of his mentors and the experience in research while at Fisk crystallized Walker's career interests and colored much of his later work.

Walker's knowledge and understanding of the race problem—and of human relations in general—were enriched by his experiences as a member of the professional staff of various community agencies engaged in action programs in the city of Chicago during the mid-1940s. These agencies included the Southside Planning Board, the American Council on Race Relations, and the Mayor's Commission on Human Relations. In 1947–1948 he served as secretary of the Committee on Education, Training and Research in Race Relations at the University of Chicago. Walker's keen insight into the problems of race relations led many organizations with programs in intergroup relations to seek his services as a consultant. He served at various periods as a consultant for government agen-

cies, the Southern Regional Council, the Commission on Race and Housing, the Fund for the Advancement of Education, and the NAACP.

Professor Walker was heavily involved in the work of the sociological fraternity. He served as president of the District of Columbia Sociological Society in 1952–1953 and was elected to serve again as president for 1959–1960. He had been a member of important committees of the American and Eastern sociological societies, and had presented papers before the American and District sociological societies, the Association for Public Opinion Research, and the Third World Congress of Sociology. His publications include *The Negro in American Life* (1954).

Harry Walker was a warm and friendly human being, of keen intelligence and sharp wit. His passing deprives the academic profession and the sociological fraternity of an able representative and the broader community of a devoted public servant. He will be sorely missed by those who knew him intimately and shared his fellowship.

G. FRANKLIN EDWARDS

Howard University

Sections of the American Sociological Association

Section on Methodology: Officers for 1960 include Albert J. Reiss, Jr., Chairman; Daniel O. Price, Chairman-Elect; Robert McGinnis, Secretary-Treasurer; and Edgar F. Borgatta, Sanford M. Dornbusch, Patricia L. Kendall, Leslie Kisl, George A. Lundberg, and Shirley A. Star, members of the Council.

Section on Medical Sociology: Officers for 1960 include August B. Hollingshead, Chairman; Odin W. Anderson, Chairman-Elect; Samuel W. Bloom, Secretary-Treasurer; and Everett C. Hughes, Benjamin D. Paul, and George G. Reader, members of the Council.

International Association of Gerontology. The Social Research Committee will hold an International Seminar on Social and Psychological Aspects of Aging in August, 1960, just prior to the Fifth International Gerontological Congress. Further information is available from Clark Tibbitts, Special Staff on Aging, U. S. Department of Health, Education, and Welfare, Washington 25, D. C.

International Society for the History of Ideas. This Society, organized last year by the *Journal of the History of Ideas* to arrange for inter-

national and regional meetings and seminars of individual scholars, announces the following officers for this year: Hans Kohn, President; Herbert Butterfield, Jean-Baptiste Duroselle, Howard Mumford Jones, Philip P. Wiener, Vice-Presidents; Aaron Noland, Secretary-Treasurer; Stephen Toulmin, Associate Secretary; and Arthur O. Lovejoy, Honorary President. The office of the Society is located at 137 Finley Center, The City College, New York 31, New York.

The Community Council of Houston. Gwynn Nettler has resigned as Director of the Child Welfare Study which the Council is conducting under grants from the W. B. Sharp Foundation and the Hogg Foundation for Mental Health. He is returning to the practice of industrial psychology in Mexico City.

Council of Jewish Federations and Welfare Funds. Harry L. Lurie Fellowships for Social Work Education and Research are available for qualified persons as educational fellowships or research grants. Application for either type of grant should be filed no later than February 15, 1960. For further information write to the Council, 729 Seventh Avenue, New York 19, New York.

Eastern Sociological Society. The 1960 meeting of the Eastern Sociological Society will be held at the Somerset Hotel, Boston, on April 23rd and 24th. As the theme of the meetings, President William J. Goode has chosen "Evaluation of the Decade, 1950-1960," with the emphasis upon sociological aspects of economic growth and development. Albert Ullman of Tufts University is Chairman of Local Arrangements.

An innovation of the meeting will be the presentation of the Society's Merit Award of 250 dollars. This is to be given annually to an elder and distinguished statesman of the field, not for a particular piece of work, but to a present or past member of the Society who has worked for a substantial part of his career in the area represented by the Eastern Society and who has taken part in the Society's meetings or deliberations. Harold Pfautz is chairman of the Committee on the Profession which is charged with recommending to the Executive Committee its choice for the award.

In addition to President Goode, officers elected for the current year are A. B. Hollingshead, President-elect, Clyde V. Kiser, Vice President, and Jerome K. Myers, Member of the Executive Committee. Wilbert Moore continues as E.S.S. Representative to the A.S.A. Council and Kurt Mayer and Bartlett Stoodley as Executive Committee Members. Austin Van der Slice resigned as Secretary-Treasurer to spend a sabbatical leave abroad. The post was filled by the appointment of Elizabeth Briant Lee.

National Science Foundation. Expenditures for scientific research and development by private philanthropic foundations and voluntary health agencies totaled 95 million dollars during 1957. About 59 million dollars of this amount was in support of basic research, according to a Foundation report, "Research and Development Expenditures of Foundations and Health Agencies, 1957,"

No. 15 of the series *Reviews of Data on Research and Development*. Copies may be obtained from the National Science Foundation, Washington 25, D.C.

Limited funds are available for the support of travel by American scientists to international scientific congresses. The closing date for receipt of applications is February 15, 1960; forms may be obtained from the Foundation, Washington 25, D.C.

Social Research, Inc. announces that Horst Finkemeyer, social psychologist on the staff of its German affiliate, The Institut für Absatzpsychologie, Hamburg, will be associated with SRI for a six-month period in order to become acquainted with American methods in the study of consumer behavior. As part of this program, Mrs. Harriett Bruce Moore, Associate Director of SRI, is currently serving as Director of the Hamburg Institut.

U. S. Public Health Service, Division of Nursing Resources. The Surgeon General has announced ten new awards for extramural research in nursing. The recipients, who comprise nurse faculty members, a public health nurse, social scientists, and a psychologist, will carry out their projects at public and voluntary agencies and university schools of nursing.

Wisconsin Sociological Association. At the first annual meeting held on November 7, 1959 in Madison, Wisconsin, the following were elected to the executive committee for the year 1959-1960: President, Albert Blumenthal, Wisconsin State College, Eau Claire; Vice-President, Tom Stine, Carroll College, Waukesha; Secretary-Treasurer, Sister M. Rebecca, O.S.F., Alverno College, Milwaukee; Editor, Hugo O. Engelmann, University of Wisconsin-Milwaukee; and Member-at-Large, Robert Davis, University of Wisconsin-Madison.

University of Alberta (Department of Philosophy and Psychology). Gordon K. Hirabayashi, formerly of the American University at Cairo, has joined the staff as Associate Professor of Sociology and John Hostetler has been appointed Assistant Professor.

Robert W. Jones, presently research director of the Alcoholism Foundation of Alberta, and Amal Vinogradov have been appointed special part-time sessional instructors in Sociology for the year 1959-1960.

University of Arkansas. Dean Johnson, formerly Associate Director of the Family Counseling Service, Menninger Foundation, has been appointed Associate Professor, and Gary Maranell, formerly of the State University of Iowa, has been appointed Assistant Professor. Hans Hoffman is Visiting Professor of Anthropology. Fred W. Vogt is on leave and serving as Visiting Professor of Anthropology at the University of Toronto.

Boston University. The first issue of the monthly bulletin of the research division of the Arts Center, *The Arts and the Behavioral Sciences*, has been published. Editor Donald S. Bloch, Assistant Professor in Social Science at the School of Fine and

Applied Arts, and member of the research policy board of the Center, states that the bulletin will include reports of ongoing research programs at the Arts Center and at other institutions, announcements of special events related to the arts in American society, and lists and reviews of new works in the field. Interested persons can be placed on the bulletin mailing list by writing to Dr. Max Kaplan, Arts Center Director.

Campbellsville College, Kentucky. Fred R. Yoder, formerly of Washington State University, has been appointed Chairman of the Division of Social Sciences. Wilma P. Yoder, has been appointed Instructor in Sociology.

Carleton College. William L. Kolb, formerly of Tulane University, has been appointed Fred B. Hill Professor of Sociology and Chairman of the Department of Sociology and Anthropology. Russell L. Langworthy has been promoted to the rank of Associate Professor.

Catholic University of America. Establishment of a Bureau of Social Research is announced, which will be administered under the University's Department of Sociology. Paul Hanly Furley, head of the Department, will be Director of the Bureau. Under a National Institutes of Health grant, Furley is conducting a study of marginal employability within the District of Columbia, and Thomas J. Harte is continuing a government-sponsored project in genetics of an isolated group living in a southern Maryland community.

Central Michigan University. The Sociology section of the Department of Social Sciences has now been given departmental autonomy. The new Department, headed by Bernard N. Meltzer, also includes the following members: Carl J. Couch, Harry R. Doby, Ernest P. Donald (visiting), Theodore N. Ferdinand, Sherman L. Ricards, Robert L. Stewart, and Charles M. Westie.

Chatham College. William C. Lehmann has been appointed Mary Helen Marks Visiting Professor for 1959-1960. He will serve as Professor of Sociology and Chairman of the Department in the place of Mabel Elliott, who is on sabbatical leave this year.

University of Chicago. The Sociology and Psychology Departments jointly have received funds from the National Institutes of Health for a training program in Social Psychology. Students who have completed at least one year of graduate training with distinction and anticipate taking their doctorate in either the Sociology or Psychology Departments are eligible. Cost of living fellowships over and above tuition and fees will be provided students accepted in the program. Persons interested should so indicate on their application for admission and fellowship assistance when submitted to the Graduate School and the department concerned. The program officially began in October, 1959. Fred L. Strodbeck, now a Fellow at the Center for the Advanced Study in the Behavioral Sciences, will act as training director. James A. Davis, Department of

Sociology, will answer inquiries concerning the program pending Professor Strodbeck's return in October, 1960.

University of Connecticut. A Ph.D. program in sociology has been established in the Department of Sociology and Anthropology.

Harris Chaiklin, a graduate student at Yale University, has joined the teaching staff. Claus B. Bahnson has joined the research staff to work on a further grant to Walter I. Wardwell from the Institute of Mental Health to study the social and psychological factors in the etiology of coronary heart disease. Arthur J. Vidich is conducting a study on the social and psychological consequences of change in rural Puerto Rico sponsored by the Social Science Research Center of the University of Puerto Rico and supported in part by the National Institutes of Health. Bernard C. Rosen is on sabbatical leave for the first semester, 1959-1960. Donald P. Kent has been designated by Governor Kibicoff to direct Connecticut's preparation for the White House Conference on Aging to be held in January, 1961.

In the Department of Rural Sociology, Philip J. Olson of Purdue University and Donald J. Hay from the Department of Agriculture have joined the research staff. Robert J. Burnight was on leave during the fall semester to study population migration in Mexico on a Guggenheim Fellowship.

Bowling Green State University. Joseph Perry, formerly Acting Junior Sociologist at Washington State University, has joined the staff.

Golden Gate College. James F. Carey was recently appointed Director of General Studies. He was formerly affiliated with the Center for the Study of Liberal Education for Adults.

University of Illinois. Louis Schneider, formerly of Purdue University, has been appointed Professor of Sociology and Head of the Department. He will begin his duties on March 15, 1960. At present, he is Visiting Professor of Sociology at Dartmouth College.

On July 1, 1959, the Anthropology Division of the Department was separated and established as an independent Department of Anthropology. Julian Steward is serving as Acting Head of the new Department.

J. E. Hulett, Jr., who has served as Chairman of the Department since 1953, has been granted a sabbatical leave of absence for the second semester of the current academic year, which he will spend at Princeton University.

J. W. Albig, who has been on the staff of the Department for the past thirty years and served as Chairman from 1940 to 1953, has announced his intention to retire at the end of the current academic year.

Two members of the department retired on August 31, 1959: Associate Professors Ernest H. Shideler, and Benjamin F. Timmons.

Joseph R. Gusfield has been promoted to the rank of Associate Professor.

Bennett M. Berger, formerly of the University of California at Berkeley, has been appointed Assistant Professor in the Department and Research

Assistant Professor in the Institute on Communications Research.

Norman W. Painter, formerly Professor of Sociology at the Central University of Venezuela in Caracas, has been appointed Lecturer in Sociology.

Kansas State University. Randall C. Hill, Professor, Visiting Professor at Utah State University last summer, recently returned from a two-year I.C.A. appointment in India.

Linwood L. Hodgdon, Professor, has returned from a year's leave of absence spent in India as a Fulbright lecturer at Agra University.

Ralph E. Dakin, recently promoted to Associate Professor, has been engaged in research on social variables associated with leadership effectiveness for the University Agricultural Experiment Station. He has also designed a survey of planning and services in Kansas communities for the Kansas Council of Children and Youth.

Glenn Long, Assistant Professor, is representing the University on a state welfare planning committee sponsored by the Kansas State Department of Social Welfare.

Wayne C. Rohrer, from the University of Maryland, has joined the staff as Associate Professor in the Department of Economics and Sociology.

University of Kentucky. Howard W. Beers has resigned as Head of the Departments of Sociology and Rural Sociology and A. Lee Coleman has been appointed to succeed him. Beers, who has been in India since June, 1958, on a Ford Foundation assignment, has accepted appointment as a community development consultant on the staff of the Council on Economic and Cultural Affairs, remaining on leave from the University as Distinguished Professor of Rural Sociology. Coleman returned on July 1 from sabbatical leave spent at the University of North Carolina, Chapel Hill.

Willis A. Sutton, Jr., Associate Professor of Sociology and Executive Director of the Bureau of Community Service, is spending the current academic year in India on a Ford Foundation appointment. He will teach in the Central Institute for Study and Research in Community Development, Missouri.

Jiri Kolaja, Assistant Professor of Sociology, spent the summer in Yugoslavia, doing research in industrial sociology under a Social Science Research Council grant.

The tenth annual Seminar in Intergroup Relations was held during the summer session, in cooperation with the National Conference of Christians and Jews. Willis A. Sutton, Jr., was Director and Edsel Godby, Associate Director.

Robert A. Danley, Assistant Professor of Rural Sociology, has resigned to accept a position at Cornell University.

Louisiana State University. The School of Social Welfare has appointed Charles S. Prigmore as Associate Professor.

Mankato State College. T. R. Schaffler has returned from a sabbatical year, spent in the subcontinent of Asia, where he served as a member of

a consulting educational workshop team on a Fulbright assignment to Pakistan.

John M. Hunnicutt, from William Jewell College, Missouri, and Mrs. Erma Gorder, from South Dakota State College, have recently joined the sociology staff.

University of Massachusetts. Mrs. Grace Harris has been appointed Assistant Professor of Anthropology, to replace Marc Swartz, who has accepted a similar position at the University of Chicago. Edwin D. Driver, Thomas O. Wilkinson, and Lewis Yablonsky have been promoted to the rank of Associate Professor. Driver is completing research on differential fertility in central India with the aid of a grant from the Population Council, and Wilkinson a monograph for International Urban Research, University of California, Berkeley. Yablonsky has been named the winner of the 1959 Helen L. DeRoy Award by the Society for the Study of Social Problems for his paper, "The Delinquent Gang as a Near-Group."

C. Wendell King is on a year's sabbatical leave in Jamaica, B.W.I., where he is engaged in a research project in social change, supported by a grant from the Society for the Investigation of Human Ecology.

A number of fellowships for Master's degree candidates have been made available. Information may be obtained by writing to the Department of Sociology and Anthropology.

Michigan State University. Margaret Read, formerly of the London School of Economics, will be a Distinguished Visiting Professor during the spring term.

New appointments to the staff include: James McKee, who has a joint appointment in the Department and in the Institute for Community Development and Services, and Warren Sauer.

J. Allan Beegle, William H. Form, and Paul Honigsheim attended the World Congress of Sociology meeting in Stresa, Italy, in September. Form and Beegle were given travel grants by the National Science Foundation.

Richard N. Adams has been elected to the Executive Committee of the Society of Applied Anthropology.

William H. Form has been appointed Associate Director of the Labor and Industrial Relations Center. He will be in charge of research and planning at the Center, and will continue to teach and do research in the Department of Sociology and Anthropology.

Iwao Ishino has returned from a Fulbright Lectureship at the University of Tokyo. While there he collaborated with John D. Donoghue in a study of cultural change in twelve communities.

Robert C. Hanson has returned from a year of research on community power structure and hospital-community relations in Denver, Colorado, and David Lewis from Japan where he collected data for a study of the mobility orientations of youth in the Tokyo area.

University of Minnesota. Robert Forman has been appointed to the Jane Addams Chair of

Sociology, replacing Ruth Schoenle Cavan, who has retired.

Arnold Rose has been appointed by Governor Orville Freeman as Chairman of Minnesota's steering committee planning for the White House Conference on Aging in 1961. He also was appointed to the Committee to revise the City Charter of Minneapolis.

Reuben Hill participated in the annual summer school of the National Marriage Council in London, and in the Fourth World Congress of Sociology in Stresa.

Roy G. Francis has been appointed Executive Secretary of the Social Science Research Center.

The Duluth Branch of the University reports that A. Neil Pearson, Head of the Department, has been promoted to the rank of Professor, and that Holger R. Stub has received a grant-in-aid from the Graduate School to study migration to Duluth.

University of Missouri. Robert Habenstein is on sabbatical leave this year, and is being replaced by George Zollschan, who has been engaged in research at the State Hospital in Topeka, Kansas.

C. T. Pihlblad worked on a migration study sponsored by Community Studies, Inc., last summer.

Dale Henning, of the University of Iowa, has been appointed Instructor in Anthropology and Director of the Museum of Anthropology, replacing Richard Keshin, who has taken a position at Beloit College. Ansel Simpson, Gene Acuff, and Samuel Yoshioka have been appointed Instructors in Sociology.

Charles S. Henderson has returned from a year in India under a Fulbright grant, where he was engaged in a study of the Anglo-Indian community, and Noel P. Gist has returned from the Netherlands, where he was Fulbright Lecturer at the Institute of Sociology, University of Groningen.

James Greenlaw has been appointed to the sociology staff of William Woods College at Fulton, Missouri, and Richard Brown has accepted an appointment as Chairman of the Department of Sociology at Missouri Valley College.

Oregon State College. Robert H. Dann, Professor Emeritus, died August 18, 1959. He had been on the staff from 1927 to 1956. During the past two years he was Visiting Professor at the College of the Pacific. (See p. 109 of this issue of the *Review*).

Arthur E. Gravatt, on leave from Willamette University, and Bernard L. McCarthy, formerly of Wisconsin State College, have joined the staff as part-time Instructors.

Park College. Wayne Wheeler spent the summer in Scandinavia doing research in social organization. In September, he attended the Fourth World Congress of Sociology in Stresa.

University of Pennsylvania. Thorsten Sellin has relinquished his post as Chairman of the Department but is continuing as Professor of Sociology and Editor of *The Annals*. Under a Fulbright grant

he is spending the academic year at Cambridge University (England) as Visiting Lecturer.

Vincent H. Whitney, formerly of Brown University, has accepted appointment as Professor of Sociology and Chairman of the Department. During the summer he was a participant in the meetings of the International Union for the Scientific Study of Population in Vienna, and of the International Sociological Association at Stresa, under a travel grant from the Social Science Research Council.

Dorothy Swaine Thomas gave the presidential address at the 1959 annual meeting of the Population Association of America in Providence. She received a travel grant from Social Science Research Council to attend the meetings of the International Union for the Scientific Study of Population in Vienna.

Otto Pollak is working with Family Service of Philadelphia in the construction of a quantitative study of family relations. He is also serving as consultant to the Behavioral Research Department of Lankenau Hospital.

Everett S. Lee is spending one year at the Institute für Weltwirtschaft in Kiel, Germany, conducting research on refugee migration and occupational adjustment under a grant from the Population Council.

Marvin E. Wolfgang has been promoted to Associate Professor. He was elected a member of the Executive Board of the Pennsylvania Prison Society.

William Kephart has received a grant from the Carnegie Corporation to study the role of liberal arts in business education.

E. Digby Baltzell spent the past year in Spain and other European countries working on a study of Tocqueville.

Hope Tisdale Eldridge has joined the staff of Population Redistribution Studies as Research Associate. Her book, *The Materials of Demography*, has been issued jointly by the International Union for the Scientific Study of Population and the Population Association of America.

Harry Bash is analyzing migration differentials in mental disease under a grant from the Research Foundation for Mental Hygiene.

Ann Ratner Miller, Research Associate on the staff of Population Redistribution Studies, is completing a study of labor force age, sex, nativity, and color differentials under a grant from the National Science Foundation.

Norman Johnston spent last year in England and the Continent on a Fulbright grant and Faculty Research grant, working on an historical study of prison architecture.

Princeton University. The staff of the Sociology and Anthropology section of the Department of Economics and Sociology includes: Gerald Breese, Marion J. Levy, Jr., Wilbert E. Moore, Frank W. Notestein, and Frederick F. Stephan, Professors; Jessie Bernard and W. D. Borrie, Visiting Professors; Morroe Berger and Melvin Tumin, Associate Professors; Heinz Hartmann and Edward Tiryakian, Assistant Professors; R. Leighton Van Nort, Visiting Lecturer; and Peter Kunstadter and Malcolm Willison, Instructors.

Charles Page, former Chairman of the Department of Sociology and Anthropology at Smith College, will become Professor of Sociology and assume the duties of executive officer, beginning September 1960.

Jessie Bernard, on leave from Pennsylvania State University, is currently conducting a study of the application of game theory to sociological theory.

W. D. Borrie, Head of the Department of Demography at the Australian National University at Canberra, is spending the current academic year at the Office of Population Research as Visiting Professor of Demography and is serving as general adviser on research.

Frank Notestein has resigned as Professor of Demography and Director of the Office of Population Research in order to assume the Presidency of the Population Council. During the current year he will continue to offer graduate work in demography at Princeton. Ansley J. Coale has been appointed Director of the Office of Population Research.

Frederick Stephan, Editor of *Public Opinion Quarterly*, is chairman of the Social Statistics Section of the American Statistical Association, adviser to the Census Bureau, and member of the Advisory Committee on Statistical Policy for the Office of Statistical Standards of the Bureau of the Budget.

Willbert Moore is on leave of absence under a fellowship of the Center for International Studies at Princeton, completing the study, "Industrialization: The Permanent Revolution." He is also inaugurating a two-year UNESCO project concerned with social aspects of industrialization. Moore is a member of the SSRC Committee on Economic Growth, and has been elected to the Board of Directors of the SSRC.

Gerald Breese has recently returned from India, where he served as Director of the Stateside Team of the Ford Foundation Planning Consultants. In that capacity, he assisted the Town Planning Organization of the Government of India in the preparation of a Master Plan for the Greater Delhi Area. He continues as Director of the Bureau of Urban Research at Princeton.

Marion J. Levy, Jr., has been awarded a Ford Foundation Fellowship for a study of Japanese social structure and modernization. He is a member of the editorial board of *Behavioral Science*, the advisory board of the *Journal of Asian Studies*, and the Board of Directors of the Human Relations Area Files.

Morroe Berger has returned from a year of field work in Egypt and other Middle Eastern countries, sponsored by the Near East Program of Princeton. As an Associate of the Center of International Studies at Princeton he has recently completed a study of the military elite as a vehicle of social change in Egypt since Napoleon.

Melvin Tumin has completed a five-year project on social class and social change in Puerto Rico. As a Law Fellow of the University of Wisconsin, he participated last summer, under the auspices of the Ford Foundation, in a seminar on "Implementation of Unpopular Decisions." He is a member of the Editorial Board of the *Public Opinion Quarterly* and Chairman of the Committee on Intergroup

Relations of the Society for the Study of Social Problems.

Edward Tiryakian has returned from a field trip through Africa, under terms of a grant from the Sub-Sahara Studies Program at Princeton. He is on leave this year as a Bicentennial Fellow of Princeton University to continue his studies in the sociology of religion.

Heinz Hartmann has recently completed a study of problems connected with the export of industrial culture. He is a Research Associate of the Industrial Relations Section of Princeton University.

R. Leighton Van Nort, on leave from Bowdoin College, is serving as Visiting Lecturer and is engaged in a reevaluation of the theory of demographic transition.

Peter Kunstader, formerly of the University of Arizona, is engaged in a study of the use of modern medical facilities by Mescalero Apaches.

Malcolm Willison, formerly of the College of William and Mary, is presently completing a study of the role of the elite in Indonesian Society.

The Sociology and Anthropology Section now offers a program of studies leading to the Ph.D. in Sociology, with a supporting minor concentration in Anthropology. The Office of Population Research, the Bureau of Urban Research, The Center of International Studies, and the Industrial Relations Section at Princeton provide opportunities for advanced research training in specialized fields. Inquiries concerning fellowships and scholarships for graduate study should be addressed to Melvin Tumin, Departmental Representative for Graduate Work.

Rutgers, The State University. Ludwig L. Geismar, formerly research associate with the Family Centered Project in St. Paul, Minnesota, has been appointed Associate Professor in charge of research at the Graduate School of Social Work.

Stetson University. Sidney B. Denman has been promoted to the rank of Professor of Social Psychology and named Chairman of the Department of Sociology.

The University of Texas. Henry Bowman and Walter Firey have been advanced to the Professorship. Firey has been appointed a Fellow at the Center for Advanced Study in the Behavioral Sciences for 1959-1960.

Leonard Broom has been appointed Professor and Chairman of the Department, succeeding Warner E. Gettys, who continues as Professor of Sociology.

Richard Colvard has been promoted to the Assistant Professorship.

Jack P. Gibbs has been appointed Assistant Professor, William J. Millard part-time Lecturer, and Cletus Brady Instructor.

Harry E. Moore is editor of the *Southwestern Social Science Quarterly*.

Wayne State University. Kathleen Gough has been appointed as Visiting Lecturer in Anthropology; Walter Kaufman and Denez Gulyas have been appointed research associates; Arnold Pilling has been promoted to Assistant Professor, and

Helen Hausé has rejoined the staff as Assistant Professor.

Eleanor Wolf is on leave at the Merrill-Palmer School, and Frank E. Hartung is on leave at the University of Wisconsin.

Leonard W. Moss received an ACLS Grant and a Wayne State Fund grant-in-aid for participation last summer in the Fourth World Congress of Sociology and the First World Congress for Jewish Folklore Research, Tel-Aviv.

Jesse Pitts was the recipient of a summer grant from the SSRC for the study of French Industrial firms and French family structure.

Edward C. Jandy has been appointed by Governor Williams as chairman of a task force to study juvenile delinquency in Michigan.

The Division of the Science of Society of the newly-established Montith Experimental College is directed by Sally Cassidy. Other members of

the staff include: Andrew Gundar Frank, Martin Orans, Richard Pope, and Robert Thomas.

The University of Wisconsin-Milwaukee. Robert P. Stuckert, formerly of Ohio State University, was appointed Assistant Professor last March.

H. Yuan T'ien, a research scholar in the Department of Demography, the Australian National University, has been appointed Assistant Professor.

James Silverberg is completing a study of the relationship between caste membership and diverse peasant roles, using data gathered on two field trips to India.

Martin Cohnstaedt has been asked to serve on a subcommittee of the Mayor's Study Committee on Social Problems in the Inner Core of the City of Milwaukee.

Juris Veidemanis is directing a research project on Alcoholics Anonymous in Milwaukee.

BOOK REVIEWS

The Human Meaning of the Social Sciences.

Edited by DANIEL LERNER. New York: Meridian Books, 1959. 317 pp. \$1.45, paper.

This is an important volume of ten essays on the role of the social sciences in the modern world. Eight were originally written for the literate non-professional and were published in the French review, *Esprit*. Now expanded and revised these eight essays, with the addition of two others, have been published in America under the editorship of Daniel Lerner.

Six of the ten essays may be recommended to the reader with only the briefest mention. This is made possible by their nature and necessity by the limits of this review. Two of the six are straightforward accounts of matters with which most social scientists will already be familiar: Harry Alpert describes the recent "Growth of Social Research in the United States," and Paul A. Samuelson gives a brief account of "What Economists Know." Two others are interesting accounts of essentially single aspects of the relation of the social sciences to the public and the public weal: Harold Lasswell stresses the novel idea of a "social planetarium" as a means of bringing home social science knowledge, and Max F. Millikan explores the problems of cooperation between scientist and policy maker in his "Inquiry and Policy: The Relation of Knowledge to Action." Finally, Clyde Kluckhohn and Margaret Mead contribute what are essentially professional articles. Kluckhohn writes on "Common Humanity and Diverse Cultures" stressing his increasing interest in non-relative universal values and in the problem of bringing order into the study of values by finding a limited set of categories by means of which value systems can be classified. Mead suggests several hypotheses as to the sources, conditions, and limits of "reflective change" in her "Changing Culture: Some Observations in Primitive Societies."

The other four essays require more critical comment given the context within which they are written and the fundamental issues for the relationship of American life and social science which they raise. The picture of the role of the social sciences in modern life drawn by Nathan Glazer, Daniel Lerner, Lawrence K. Frank, and Edward A. Shils is an optimistic, humane one, stressing the empirical, quantitative, policy-relevant aspects of the various disciplines. Glazer puts his emphasis on the improvement of

social science knowledge that occurred with the switch from illustrative thinking to the gathering of great amounts of empirical data in "The Rise of Social Research in Europe." Lerner links such research with democracy and the "happy" life in answering the question "Social Science: Whence and Whither?" Similarly, Frank sees social scientists, especially psychologists, playing increasingly important roles in all our major organizations and carrying out functions of value determination and judgment earlier performed by other men. Against this, Shils offers a perceptive analysis in his "Social Inquiry and the Autonomy of the Individual" of the moral problems involved in the procedures of research—defenders of the jury room "bugging" should read his analysis of such methods—but is fundamentally optimistic because "the theory of action" which has recently come to the fore "... makes ample acknowledgment of man's moral and rational capacities."

But was the relationship of social science and human welfare ever that simple and if it was, can it be said to be so now? Glazer's narrative has as its hero Charles Booth who conducted "the first great empirical social scientific study, an investigation into the conditions of life among all the people of London." According to Glazer it was his detailed analysis of tremendous numbers of facts that made Booth's insights into the causes of the misery of the nineteenth century superior to that of Marx. Granted the importance of gathering as much data as possible relevant to the categories employed, cannot it be said that Booth's superiority lay possibly in his categories—his image of man—implicit as they were? We know that all the facts cannot be gathered; thus the criteria of selection become important. While a superficial determinism was popular in the nineteenth century, it is probable that the implicit categories employed by Booth were voluntaristic and humanistic. Can the same be said for the categories of modern social science?

According to Shils the answer to this last question is "yes," insofar as the theory of action has replaced older positivistic theories. Certainly one finds in Mead's concept of the "I" and Parsons' conception of voluntarism in *The Structure of Social Action* the idea of the actor as a freely choosing self. But it is a matter of record that the "I" has increasingly disappeared from symbolic-interactionist social psychology and that

as the theory of action has been absorbed by functionalism with its concern for the self-maintaining system—in which even the psychiatrist is viewed as primarily an instrument of social control—the category of freedom has disappeared from its vocabulary. What has appeared is the socio-culturally determined man shaped within the limits of a permissive biological inheritance.

When the deterministic image of man and the social sciences which work and act upon it encounter laws, institutions, groups, and values based upon the assumption of freedom and responsibility the result is ambiguity of the sort found on every page of Frank's "Psychology and the Social Order." One can only say "yes" to the emancipation of man from the cruelties and barbarism that often accompanied the belief in absolute responsibility. But when one "shifts the focus from the action, however deplorable, to the actor" and hence to the potential act—as in the case of possible tests to predict disloyalty—what becomes of the idea that men can be punished only for acts and not for attitudes? True, the actor is not "punished," but if indeterminate institutionalization, refusal of employment, or involuntary treatment are based on scientific diagnosis, the result may be the same. Somehow, some way, the truth that man's actions are conditioned must be reconciled or held in tension with the existence of man's freedom. It is not sufficient to guard against those scientists who are "power seekers or malevolent personalities" as Frank would do. The issue lies in the relationship between the social sciences and the free society, and within the social sciences themselves.

Such reflections render dubious Lerner's easy identification of social science and democracy. It is true that the thought control of the totalitarian societies has prevented the rise of a social science in which ideology does not dictate empirical findings. But the notion that attitude research is poles apart in its possible uses from totalitarian propaganda and personality manipulation—this seems naive in the extreme. What are we to say, for example, when we are told—contrary, incidentally, to a statement in Frank's essay—that "Even motivational research, often regarded as iniquitous invasion of a person's darkest secrets, provides information that can only be used—insofar as it provides useful information at all—to satisfy more customers more fully."

The problems sharpened by the essays in this book are not to be solved by a know-nothing attitude toward social science. If they are to be solved, it will only be by continuing conversation among social scientists, and between them and the philosophers, the theologians, the human-

ists, and the rank-and-file citizen. It is time, however, to get on with the discussion, and it can only be hoped that the *Human Meaning of the Social Sciences* will give the needed impetus.

WILLIAM L. KOLB

Carleton College

Freud and the 20th Century. Edited by BENJAMIN NELSON. New York: Meridian Books, Inc., 1958. 314 pp. \$1.45, paper.

The virtue of anthologies—variety—cannot be separated from their defects: subject matter, relevance, and, above all, disparate quality of contribution. *Freud and the 20th Century*, however, has a number of pieces each of which is good enough to make the book well worth its price.

Quite the most remarkable is Abraham Kaplan's "Freud and Modern Philosophy": its sustained brilliance sags a little only toward the end when Kaplan is tempted to make Freud an honorary member of the Optimists Club. (However, Kaplan is too honest to succeed.) Another first-rate essay is Niebuhr's: solid, thoughtful, original, and to the point; in this book, it is one of the few essays seriously tackling Freud's social relevance. For, although (or because?) edited by a sociologist-historian, no sociologist or historian contributes to the volume. Freud's relevance to social matters is dealt with mainly by *obiter scripta*.

In this respect, the book does not live up to its title. There is little on the development of Freud's work in the 20th Century. There is, it is true, Herberg's fine critical analysis of what may be called vulgar (neo-) Freudianism. But there is no sympathetic yet critical stock-taking—no discussion, for instance, of whatever progress has been made to make Freud's theories testable, nor of Freud's Lamarckism, nor of the reformulation of his social theories (which probably has avoided some of the most damaging criticisms). There is no discussion of the development of Ego theory which is the major 20th century development in psychoanalysis. In short, the 20th century is prominent only in the title.

Let us return to what Professor Nelson does offer. Stanley Edgar Hyman's and E. H. Gombrich's essays each contain original insights contributing to our understanding of something—even if not always of Freud. Erik H. Erikson manages to combine an interesting piece on Freud with a short introduction to psychoanalysis. Many of the other essays are par for the course, though not necessarily breathtaking.

Some should never have seen the light of day. Maritain's essay suggests that even an erudite philosopher should not write on subjects with which he is patently unfamiliar: the psycho-

analysis Maritain attacks never existed on land or sea. Hacker's essay on "Freud, Marx and Kierkegaard" is sophomoric; Nigel Walker's "A New Copernicus?" smashes with great aplomb into a number of open doors. The reminiscences of Weizsaecker and Kardiner are barren alike of amusement and instruction.

The trouble with anthologies is that a minimum number of pages must be reached; and a minimum number of "names" must be included. I think under these circumstances—which are beyond any anthologist's control—Professor Nelson did nobly by finding and making available a remarkable number of first-rate pieces.

ERNEST VAN DEN HAAG

New York University and
The New School for Social Research

Thorstein Veblen: A Critical Reappraisal. Edited by DOUGLAS F. DOWD. Lectures and Essays Commemorating the Hundredth Anniversary of Veblen's Birth. Ithaca: Cornell University Press, 1958. 328 pp. \$5.00.

On the occasion of the hundredth anniversary of Veblen's birth in 1857, the Department of Economics at Cornell, recalling also that he had made his formal entry into economics at that institution, designed the present *Festschrift* for him. Seventeen items are included herein, and they reveal marked diversity, yet a diversity that is understandable given the variety of Veblen's own interests.

Joseph Dorfman contributes an eloquent introductory essay on the source and impact of Veblen's thought. Walton Hamilton follows with an article on "Veblen—Then and Now" which, in its free-wheeling way, is reminiscent of the provocative essay on institutions which Hamilton wrote for the *Encyclopedia of the Social Sciences*. Clarence Ayres elaborates a distinction between "workmanship" and "sportsmanship" ("sportsmanship" being loosely but effectively coalescent with "ceremonialism" and "supernaturalism") which he takes to be an equivalent of "the antithesis of reality and illusion." Comment must be sparing, and it is perhaps "unsportsmanlike" to single out this essay for special notice, but it has to be said that Ayres takes Veblen at his flattest and worst and develops this distinction in a fashion sociologically quite untenable in that it would make "ceremonialism" solely a matter of "illusions"—to be rejected without any regard for its functions. (It is of interest that Myron Watkins states in his paper in this volume that the man who knew Veblen as intimately "as it was possible to know him" was convinced that he was a "mystic." This surprises me somewhat, but it must be positively distressing to Professor Ayres.)

In "Idle Curiosity," Norman Kaplan urges that Veblen has furnished a "useful point of departure" for studies of our educational structure and of the conduct of research within it. Morris Copeland reviews some of Veblen's work on the notion that the conception of economics as an "evolutionary" science is fruitful. Lawrence Nabers presents an essay that fits together rather conscientiously the elements of Veblen's criticism of "orthodox economics." Melvin Brockie comments on the cycle theories of Veblen and Keynes. Forest Hill then compares Veblen and Marx, with careful intent but under handicapping limitations of space. Allan Gruchy, writing on the economics of growth, stresses long-term changes in "underlying economic frameworks." Paul Sweezy rejects the view that Veblen is anti-theoretical and regards him as one of a special breed of theorists that also includes such men as Marx and Schumpeter. Sweezy then offers, briefly, an integration of Veblen's theoretical scheme, and closes with an indication of what he regards as its remediable shortcomings. Joel Dirlam writes on corporation finance in Veblen's economics, with an eye to developments since Veblen's day. Leslie Fishman is concerned with Veblen's influence on Hoxie and with Hoxie's typology of unionism (business unionism, uplift unionism, revolutionary unionism, predatory unionism) in a paper which might well have profited from recent sociological work. Philip Morrison, Cornell physicist, contributes one of the more stimulating items on "The Ideology of the Engineers," in which he insists that the technologist who is "typical" and important today is not so much a self-consciously skilled fellow who wishes, perhaps somewhat contemptuously, to keep incompetents out of the way, as he is an "excited" young man from Pittsburgh or London or Leningrad who has been intrigued by the rhythms of a mighty, new, and quite unprecedentedly innovative technology. Myron Watkins reviews Veblen's notions of cultural evolution. Carter Goodrich affords a well-reasoned consideration of the "case of the new countries" (Australia, New Zealand, Spanish America), initially motivated by Veblen's idea of the "merits of borrowing." The editor, Dowd, follows neatly with a consideration of technology and social change in Japan and Soviet Russia. G. W. Zinke ends the volume with an essay on Veblen's "macroinstitutionalism," an intriguing term, certainly, but one which is left in a disappointingly vague state when Professor Zinke has gotten through his allotted pages.

Sociologists are likely to be most interested in the contributions by Kaplan, Fishman, Morrison, Goodrich, and Dowd, although several of the other papers may also prove of interest to

them. It is, of course, merely fair to note that the papers were written mostly by economists, and evidently mostly with a consuming public of economists in view. Yet there is much that is omitted here that a sociologist might have liked to see included. Even Veblen's best friends do not claim that he was a systematic thinker. (Some, indeed, see it as a virtue that he was not.) Thus, Sweezy, who goes so far as to call him "a towering genius," also refers to his "unsystematic—and often highly frustrating—way of theorizing." But, systematic or not, Veblen had a good deal to say that is still worthy of reflection and investigation in the fields of social stratification, of institutional interrelations, and of what I may call the disjunction of cultural indices from the things they "originally" indicate (as when, to give but one example—for this field is almost a specialty of Veblen's—a conservative ideology is loosed from its "original" attachments and comes to be adopted by classes whose interests the ideology does not promote in veiled fashion). In the present volume, one of these suggested fields is not touched upon at all, and the others are treated in a manner likely to leave the sociologist dissatisfied. However, there are other materials on Veblen, and this is unlikely to be the last volume that deals with his work.

LOUIS SCHNEIDER

Purdue University

From History to Sociology: The Transition in German Historical Thinking. By CARLO ANTONI. Translated by HAYDEN V. WHITE. Detroit: Wayne State University Press, 1959. xxviii, 249 pp. \$4.50.

German thought has repeatedly exerted great influence on American thought, especially in philosophy and in the social sciences. Likewise, German thought has exerted great influence on Italian thought. In the present volume, we have German thought in the period immediately preceding Hitler's rise to power explained to Italian readers (Antoni's original volume, *Dallo Storicismo alla Sociologia*, appeared by G. C. Sansoni in Florence in 1940), and then the Italian refraction of German thought, with the usual time lag, presented to the American public. This roundabout procedure has meaning. In 1940, the supreme ruler of the Italian philosophical scene, Benedetto Croce, himself heir to Hegelian modes of thinking, was still alive and Antoni was one of his disciples. It is well known that Croce, who has contributed a laudatory "foreword" to Antoni's book, was strictly opposed to any "positivistic" or "scientistic" interpretation of human action and destiny and hence to "the laborious construction of types or classes

of facts" in substitution for the concreteness of "universal-individual reality." The construction of types was identified with sociology and Antoni's report, baring the alleged weaknesses of the typological approach, was hailed by the formidable old philosopher as a vindication of his own, long-held, point of view. This translation of Antoni's report on German historians, then, confronts American sociologists with their most formidable challenger, Benedetto Croce. Here is our Gettysburg; here we must take our stand.

Carlo Antoni's original essays, examining "the transition or decline of German thought from historicism to typological sociology," were concerned with Wilhelm Dilthey, Ernst Troeltsch, Friedrich Meinecke, and Max Weber. To these were later added the Dutch historian Johan Huizinga and the Swiss art historian Heinrich Wölfflin, not to the advantage of the inner consistency of the book. Huizinga, author of *The Waning of the Middle Ages*, has little to do with the tortuous path of German thinking in the last one hundred years and it is dubious whether he is a true typologist. Wölfflin, one of my own teachers at the University of Munich, is indeed a typologist and it might be possible to construct a sociology of art on the basis of his ethno-regional categories of "seeing," but to do justice to him as a "sociologist" would have required the confrontation of these categories with the more inclusive Weberian concepts dealing with development of modern economic society. But from Dilthey to Weber, there is one grand sweep of intellectual transformation. As in a volcanic landscape, one witnesses repeated tremors and eruptions, until the mountainous figure of Max Weber emerges, full of foreboding and overshadowing everything else. Antoni's essays on Dilthey, Troeltsch, and Meinecke are enlightening; his essay on Weber is masterly. To post-Parsonian Americans, Weber is the originator of a sociology of action, but this is at best an oversimplification, quite likely a misconception. It seems to me that a means to an end is thereby severed from its context—the crisis of modern society—as well as from the intended end itself, namely an analysis of the forces which are to carry this particular society to the crest of its development, be it glory or ruin. Viewed in this way, Weber remains a historian while Parsons leads the process of de-historization, which is merely adumbrated in Weber's concepts, to its conclusion. The story has an undeniable inner logic, but the moment may have come where the premises need to be re-examined.

If there is hope in American sociology, that is, if the theoretical awakening which we believe to discern is not a rally before the demise, then Antoni's essay on Weber will have to be con-

sidered required reading for sociologists. It will help us to perceive our "looking-glass self," it will speed the socialization of sociology into the fraternity of humanistic scholarship. I do not mean to imply that we should appease the Crocean critics by abandoning any attempt at conceptualization. That is our life-blood. Rather, I mean to say that perceptive readers of Antoni's essay on Weber, in the context of the three preceding essays, might well detect in it the possibility of a synthesis. For instance, in analyzing Max Weber's sociology of the city, Antoni points out that the ideal type of the "citizen," that is, the urban middle classes in medieval northern Europe, is "useful only for the analysis of this population in its historical individuality and as such it is no longer ideal but real," and he concludes that sociology is thereby "resolved into history." To which one merely would have to add that the possible coincidence of the ideal and the real at one particular instance in history does not invalidate the usefulness of ideal-typical construction as a principle of limits in a series of other instances. In the same sense, the Ricardian scheme of rational economic action, while perhaps an observable fact at one short moment in the course of events, remains an ideal-typical model at other times and places. Antoni himself concedes the "negative" heuristic value of the ideal type. On the other hand, he notes that Weber spoke "of a casuistic sociology, of a comparative procedure," with which the historian could establish the propriety of the use of terms in his interpretation. If the historian stands in need of such a procedure, the indispensability of the sociologist is vindicated.

WERNER J. CAHNMAN

Hunter College and Yeshiva University

A Critique of Contemporary Bourgeois Sociology. By A. I. DEMIDENKO. Publication of Advanced Party School and A.O.N. attached to the Central Committee of the Communist Party of the Soviet Union. [The Chair of Dialectical and Historical Materialism.] Moscow, 1958. 72 pp. 95 kopecks, paper.

This book is much more informative on the state of social theory in the Soviet Union than it is on the "bourgeois sociology" which it purports to discuss. In fact, many a person acquainted with contemporary Western sociology would be hard put to recognize the familiar landscape as it is reflected in this Soviet "theorist's" distorted mirror.

This is the result not only of the author's expressly formulated premise that bourgeois "sociology" is simply bourgeois "ideology," but also of his conviction that "unmasking of contemporary reactionary bourgeois sociology is a

necessary condition for a successful struggle of all the progressive forces of society against the antiquated system of capitalism."

Since sociology, as practiced in the West, is not a science but an ideology, there is no reason to separate it from other ideological weapons used by the defenders of capitalism. This is, evidently, the logic which leads the author, while adhering consistently to the term "bourgeois sociology," to utilize in his survey not only the writings of sociologists and social scientists, but also those of biologists, philosophers, and theologians, as well as those of political and military leaders. This procedure leads to indiscriminate groupings and results in such strange bedfellows as G. Santayana and W. F. Ogburn, S. Freud and E. A. Ross, J. Bernard and J. Burnham, A. Toynbee and H. Hoover, F. Ratzel and J. F. Dulles.

The book consists of a long introductory chapter, describing "the characteristic traits of contemporary bourgeois sociology," followed by ten shorter chapters, discussing its "basic trends." According to the author, bourgeois sociology, which began to "decompose" as early as the middle of the 19th century, was forced by the development of historical materialism to retreat into a blind alley and to disclose in full its reactionary essence. It lost completely its scientific character and assumed the role of apologist for the tottering capitalistic system. It broke down into a multitude of schools, large and small, which argue over various non-essential issues.

Of these "schools," the author chose to discuss the following: The Psychological School, Social Darwinism, Racism, Malthusianism, The Geographical Trend, Technocracy, Sociology of the Atom, Theory of Elite, Theories of Social Stratification, Social Mobility and People's Capitalism, Microsociology, and the Sociology of the Right-Wing Socialists.

The first five of these schools are presented as serving the exploitative and imperialistic goals of the capitalist elites. The Psychological School, for example, is ready to interpret all the forms of opposition against capitalism as proofs of maladjustment and mental illness, while Neo-Malthusianism and, related to it, Psycho-Racism not only affirm permanent biological and cultural inequality of human beings, but even justify such monstrosities as sterilization laws which in the U. S. are actually used against the "progressive strata of population."

The second group of "schools" are said to be mainly the propaganda mouthpieces of capitalism. They try to conceal its real character by playing up such topics as the alleged "managerial revolution" and "people's capitalism," by misrepresenting the true meaning of social classes

and class struggle in the capitalist countries, and by trying to scare the nations of the world by a threat of atomic disaster. Behind this last stratagem there is an ambitious scheme of a world state ruled by the American businessman.

The book contains neither bibliography, nor quotations, except, of course, from the sacred literature of communism.

KONSTANTIN SYMMONS

Wilkes College

The Tower and the Abyss: An Inquiry into the Transformation of the Individual. By ERICH KÄHLER. New York: George Braziller, 1957. xv, 327 pp. \$6.00.

One tends to be suspicious at the outset of any inquiry that begins with abstractions like "the evolution and transformation of the human form," and most suspicious of all perhaps of an author who tries to diagnose the ills and prescribe the remedies for something (the largest abstraction of all) called the condition of Man. This, however, is the task set for himself by Erich Kähler, author of the earlier *Man the Measure*, in a series of lectures presented first at Black Mountain College and later at Princeton in the Christian Gauss Seminars in Criticism.

Although the abstractions remain, as one might expect from a writer trained in philosophy, history, sociology, and literature at European universities, it must be said that Kähler handles them with skill, erudition, and even, at times, with virtuosity. He is genuinely concerned with the human condition, with the fact that throughout the last three centuries at least, those things that have contributed so munificently to the comfort and ease of human beings have tended at the same time to deprive them of their humanity—and of their personalities. Science and technology, politics and psychology, have all conspired to alienate men from themselves and from their fellow men. Indeed, words like displacement, disjunction, fragmentation, estrangement, and alienation appear with undisguised and candid frequency in the pages of this book.

What happens when man is divorced from the manageable contexts of his own life, when the actor, so to speak, walks across a stage that has been stripped of all significant scenery? The answer is that we then have a man afflicted with "splits from without" and "splits from within." The external splits are those brought about by the collectivization of the individual and, especially in the case of Nazi Germany, totalization and terror. The splits from within are those the author calls "second consciousness and fractionized universe," on the one hand, and the

"new sensibility, psychoanalysis, and existentialist experience," on the other.

To our present malaise, the physical and social sciences have contributed their share. In insisting upon *Wertfreiheit* they have inevitably supported the devaluation of valuation itself. It is not so much that men deviate today from a common morality, but rather that the common morality itself has evaporated under the close scrutiny and insistent analysis of social science. The result—though Kähler does not use the word—is an anomy unlike anything that has hitherto appeared in history.

Like most social critics—and he is first rate in this respect—Kähler finds more diseases than remedies, and his diagnoses are more apparent than his prescriptions. In this last category, however, it is fair to indicate that he places above all what he regards as a necessary transformation of society from the collectivity it has become to the community it could be—and must become if we are to escape an otherwise impending doom. Thus Kähler, like Tönnies long before him, prefers the *Gemeinschaft* to the *Gesellschaft*, and it is the primeval community, free and whole, that will restore the human quality and resurrect the human form.

ROBERT BIERSTEDT

The City College of New York

Hellenism: The History of a Civilization. By ARNOLD J. TOYNBEE. New York: Oxford University Press, 1959. xii, 272 pp. \$4.50.

This frighteningly learned little book is compact and fact-filled in spite of a number of needless repetitions in the text. There is much more military and political history than exposition of the nature of Hellenic civilization, though Toynbee has many sharp and fresh things to say about the latter also. The familiar challenge-response syndrome is the principal conceptual instrument. Occasionally the author permits himself a timid incursion into psychology (e.g. p. 55). Sociologists are likely to find the first six chapters and the last three the most engaging. The intervening material is so weighted with historical minutiae that those without specialized training will get lost amid the trees.

In many respects the tone of this volume is less culture-bound than Toynbee's earlier writings. Thus he permits himself to observe (p. 16) that Mahayana Buddhism "... was even further removed from man-worship than Judaism was." On the other hand, he still lacks firm control of cross-cultural data. For instance, a *geisha* is not, strictly speaking, a "prostitute" (p. 57). Much more serious than such small lapses is his persistent obsession with Jewish-Christian religion as embodying supreme values. Constantine had

"the clear-sightedness and common sense to come to terms with Christianity" (pp. 239-40). "Hellenism's encounter with Judaism in and after 175 B.C. was the most portentous single event [*sic.*] in Hellenic history" (p. 191). Hellenism fell because of "the worship of man" and (p. 253) "the Modern World must exorcise this demon resolutely if it is to save itself from meeting with its Hellenic predecessor's fate."

Actually, Toynbee does not do justice to the counter-current in Hellenic thought. After all, the original meaning of the famous Delphic saying "Know thyself" was: remember that you are human and therefore fallible as well as mortal. Toynbee speaks only once of *hybris* (p. 15). Nor—for a balanced account of Hellenic civilization—does he give nearly enough attention to the development of Ionian science. Thales and Anaximander are casually mentioned; Anaxamines not at all; Heraclitus is merely quoted as saying that "war is the father of all things." And yet many eminent scholars have seen the world view that emerged in Ionia as the essential background to the thought of the Attic tragedians and indeed as the keystone to the distinctive features of Hellenic culture.

For all of Toynbee's learning and the sporadic flashes of wisdom that are scattered like plums in a pudding, this book will be disappointing to social scientists. Perhaps sociologists will themselves fill the gap. Apart from a few stimulating forays by Professor Howard Becker, American sociologists have too long neglected the rich data of classical antiquity.

CLYDE KLUCKHOHN

Harvard University

The Crowd in the French Revolution. By GEORGE RUDÉ. New York: Oxford University Press, 1959. viii, 267 pp. \$6.50.

Of late, there has been some interest on the part of certain sociologists in what has been called, alternatively, "historical sociology" or the "sociology of history." These interested sociologists will prick up their ears at the promises made in George Rudé's *The Crowd in the French Revolution*: Rudé promises his readers that he will abandon the traditional inclination to view the events of the Revolution and their participants from above, as it were; instead, he will use the approach to the Revolution first conceived by Jaurès—and more lately formulated by Mathiez, Labrousse, and especially Georges Lefebvre—which looks at the Revolution "from below." More specifically, he says he will examine the composition of revolutionary crowds, their aims and motives, and the processes by which they became active. Those sociologists, then, who are convinced that historical data may

be useful in shedding light on general patterns of social behavior may well be encouraged to hope that Rudé will provide grist for the mills.

In the light of this expectation, the first 177 pages of the book's 239 are rather disappointing: Rudé narrates the events of the eight *grandes journées* of the Revolution, giving in each case material on the composition, aims, etc., of the crowds, impressing the reader with the resourcefulness and thoroughness of his research, but refraining from making any general points. Hoary historical clichés about the revolutionary crowds fall by the wayside, but for the sociologist the pickings are slim. On page 178, however, begins Part III of the book, entitled "The Anatomy of the Revolutionary Crowd," and here the hopeful sociologist of history is finally rewarded.

The revolutionary crowds, says Rudé, were composed predominantly of *sansculottes* (he also often refers to them as the *menu peuple*): the workshop masters, craftsmen, wage-earners and shopkeepers, and petty traders of the capital. (Only a small fraction of them consisted of riff-raff.) They were sharply divided in social origin from their political leaders, who were drawn from the commercial bourgeoisie, the professions, or the liberal aristocracy. This discrepancy in origins produced a partial discrepancy in social and political aims: while the *sansculottes* understood and shared the aims of their leaders—aims that can, perhaps, still be most conveniently summarized as Liberty, Equality, and Fraternity—their own aims were more directly economic. Rightly or wrongly, Rudé is convinced that "the most constant motive of popular insurrection during the Revolution, as in the eighteenth century as a whole, was the compelling need of the *menu peuple* for the provision of cheap and plentiful bread and other essentials, and the necessary administrative measures to ensure it" (p. 200). The *sansculottes*, then, were not merely the passive instruments of their leaders, and it was the failure of the Mountain to satisfy the independent needs of the *sansculottes* that cost it their support, led to its downfall, and to the eventual exhaustion of revolutionary momentum. Without the leadership of the radical bourgeois the *menu peuple* could not achieve its aims in the economic sphere, and, conversely, without the support of the people of Paris, "The Republic of Virtue" collapsed.

Rudé makes a brave attempt to discuss the processes by which the *sansculottes* became indoctrinated with the ideas of the advanced political groups—through newspapers, in the National Guard, in the wineshops and markets, and especially in the clubs and societies—but his evidence is largely conjectural. And his theory that a revolutionary crowd is galvanized

into action through "panic-fear" is conjectural indeed: "the threat, real or imaginary, to three matters of vital moment—to property, life, and the means of subsistence" is said to engender such panic-fear, and while this theory may be plausible, it is hard either to prove or disprove. Never does Rudé grapple with the related, and to my mind, crucial problem of how in certain "revolutionary" periods social change is vastly accelerated because violence is institutionalized, both "from above," in the form of the Terror, and "from below," in the form of crowd action.

This shortcoming suggests, in conclusion, one of the reviewer's hobbyhorses, a long-standing interest in the problem of the unanticipated consequences of violent social action. Rudé's considerable grasp of the behavior of the revolutionary crowd might have shed much light on this general process. Here, then, is some work for the sociologist.

ELINOR G. BARBER

Columbia University

The Philosophy of Art History. By ARNOLD HAUSER. New York: Alfred A. Knopf, 1959. x, 410 pp. \$7.50.

In his monumental work, *The Social History of Art*, Professor Hauser warns that the greatest pitfall in sociological interpretation of cultural events is the use of analogies and metaphors, without concrete demonstration in the art work itself of just how such analogies are manifest. The sociologist of art needs both a theory of society based in the function of symbols as the communication of social order, and at the same time, a theory of communication in which communication is determined by our need for social order. Unless we face this we go on talking about art *and*, not *in*, society; we jump back and forth from the one to the other without really having shown that they meet at all.

Professor Hauser makes clear at the outset how we must think about the social function of art. He argues that from a sociological view a work of art is a communication which is used as:

... a tool of magic ... an instrument of animistic religion ... a magnification of the almighty gods and their earthly representatives, by hymn and panegyric, through statutes of gods and kings. Finally, in the form of more or less open propaganda, it is employed in the interests of a close group, a clique, a political party, a social class. ... The artist, whose whole livelihood, with all his hopes and prospects, depends upon such a social group, becomes quite unintentionally and unconsciously the mouthpiece of his customers and patrons (pp. 6-7).

The importance of sociology for art theory lies in the fact that it is "the focal discipline of

our day, one upon which the entire world-view of the age centers" for it decides "in favor of a rational ordering of life and for a struggle against prejudices" (p. 17). This cardinal position of sociology is founded on "the discovery of the ideological character of thought ... " (p. 17). A masterly review of various theories of art with reference to their relevance to the methodology of ideological analysis then follows. The importance or this to sociologists interested in symbolic analysis can scarcely be overstressed. Now for the first time in English we have a review of art theory from a *sociological* view. That is, the hard question is asked—and answered—How does such and such a theory help me think about the social function of art as ideology?

Professor Hauser states his position so well, and with such learning, that criticism of his position must be about the limitations of his view, not the view itself. Even if we subscribe to the image of ideology as the proper image for the relationship between art and society, difficulties remain. How do we demonstrate an ideology, or the presence of an ideological content in art? To what degree does art infuse an ideology with meaning? How do we relate the political, religious, cultural, and social aspects of an ideology? If the kind of knowledge art produces is not like the thought of the scientist, nor that of the priest, wherein does it differ? If art is a social power, how is it related to other forms of power? What is the relationship of art to social interaction in communication?

That such questions can be posed clearly after reading Professor Hauser's work indicates its value. Those who wish to get on with creating social theory based in communication, and a sociology of art with a methodology relevant to the art form itself, owe much to Professor Hauser for his incisive and comprehensive work.

HUGH DALZIEL DUNCAN

Flossmoor, Illinois

The Status Seekers: An Exploration of Class Behavior in America and the Hidden Barriers that Affect You, Your Community, Your Future. By VANCE PACKARD. New York: David McKay Co., 1959. viii, 376 pp. \$4.50.

Even more than their colleagues in related disciplines, American sociologists hold popularization in contempt. As a consequence, the broader public too often learns of the important facts and ideas in our field not from a Kenneth Galbraith, nor even from a Samuel Lubell, but from a Vance Packard. In this sense, as an object lesson, *The Status Seekers* is an important book, deserving of the close attention of every responsible sociologist.

Mr. Packard is a journalist—a person, that is, with no necessary competence to discuss America's social structure. And while he has sprinkled his pages with references to some of the relevant studies, suitably vulgarized, he has not bothered to learn what sociologists have to say on the questions he writes about. His principal sources are such dubious ones as Richard Centers and C. Wright Mills, whose special pleadings are presented in capsule form. Such sociologists are used, moreover, to lend respectability to the argument, which derives in large measure from a bewildering array of other sources: "an executive of a \$250,000,000 corporation," "a home-marketing consultant" who had conducted 411 "depth interviews," "a Dallas builder," the Color Research Institute of Chicago, "interviewers for the Chicago *Tribune* in a motivational study," "Clare Barnes, Jr., Consultant Art Director," "a waiter (named Joe)," a cow named Gertrude—even Mr. Packard himself, who spent "several weeks exploring the elite structure of a representative middle-sized metropolis."

The data culled from these informants also cover a wide range. The book is as full of assertions, if not of facts, as an almanac. We learn, for example, that "during World War II many people quit office jobs for the more glorified jobs of riveters in shipyards," that "Polish-Americans like their homes to be very garish, with loud, screaming colors," that "the Jewish man has neither the temperament nor the knowledge to putter around trimming hedges or repairing screen doors," that "people prefer to live near others as much like themselves as possible," that the call girl's pimp, "far from being a harsh exploiter, typically functions as a status symbol," that we Americans "profess admiration for intellectual pursuits, but really reserve our highest envy and respect for successful businessmen," that downwardly mobile persons stop using the word "legs" and substitute "limbs," that the reasons for the low incidence of drunkenness among Jews are thus and so, that "for the egghead, being a Democrat is the chic and daring thing to do." Indeed, there is hardly a feature of modern American society on which Mr. Packard does not express his authoritative opinion.

And these tidbits are only the hors d'oeuvres. For the main dish, Mr. Packard—to quote the book's jacket—"focuses the penetrating beam of his research and analysis" on status. In the United States, we learn, upwardly mobile persons seek recognition through status symbols. This, in Mr. Packard's opinion, is a bad thing; he goes so far as to assert that "as a result, democracy is still more of an ideal than a reality." On the other hand, he invites us to

bemoan the fate of bright boys with a "getting by philosophy," who "had been conditioned by their families not to expect much from life." "If the American Dream is to have meaning to the majority of our young people, they must be encouraged to do their best." But if they do their best and succeed in moving out of their parents' class, they become grist for Mr. Packard's penetrating motivational research.

If "status seekers" are as prevalent as Mr. Packard declares in the main body of the book, one might reasonably conclude that the United States has an open class system. But no; on the contrary, there is "a diminishing upward mobility, and a slackening of ambition." Since "the beginning of the present era (around 1940)," "our class system is starting to bear a resemblance to that which prevails in the military services." Trade unions are "developing a system of fixed 'estates' in life reminiscent of medieval guilds."

The evidence offered for this characterization is, to put it no more strongly, curious. The prior gap between manual and white-collar workers has all but disappeared: "a clear majority of all families in the \$4,000 to \$7,500 bracket, long accepted as the middle-income range, now wear blue, not white, collars." The current "gulf" is higher up, between the "diploma elite" and the rest of the society. It can be crossed only by a college graduate, and the increasing proportion of young persons attending colleges is "to some degree illusory." Automation, which by definition eliminates the tasks that can be performed mechanically, seems nevertheless to have prevented workers from rising from such jobs. The growth of bureaucracy in business, and the substitution of impersonality for family influence, has by a similar paradox further restricted the chances of those with ability but no such influence.

Mr. Packard's doleful picture of America's class system is even more disturbing when we contrast it with the past, or with other, more favored, cultures. The father of a present-day worker knew nothing of subsistence farms or sweatshops; he was "a craftsman who was proud of the barrels he made." "In older days . . . workers often developed warm friendships (or at least intense personal relationships) with their immediate superiors, and often chatted helpfully with the owner-manager." Or consider Mr. Packard's charming picture of the Spanish fishing village of San Feliu de Guixols, which he observed during a recent vacation. The barefooted fishermen worked hard on their tiny boats, but they always "seemed to be in a quietly jovial mood." At dawn each day, as they walked home with their modest share of the night's catch, "their chests were out and

their heads were high. They were pictures of men who felt pride and satisfaction in their way of life."

WILLIAM PETERSEN

University of California, Berkeley

Man and Organization: Three Problems in Human Relations in Industry. By WILLIAM FOOTE WHYTE. Homewood, Ill.: Richard D. Irwin, 1959. vii, 103 pp. \$4.50.

Men Who Manage: Fusions of Feeling and Theory in Administration. By MELVILLE DALTON. New York: John Wiley; London: Chapman & Hall, Ltd., 1959. xi, 318 pp. \$6.75.

Administrative Vitality: The Conflict With Bureaucracy. By MARSHALL E. DIMOCK. New York: Harper & Bros., 1959. xiii, 298 pp. \$5.00.

These three volumes share a common concern with the analysis of organization behavior. The relative infancy of the field is revealed in the disparate approaches used, and the non-additive character of the conclusions reached.

Whyte delivered three lectures at Dartmouth College in 1958, and here reproduces them in printed form. The first chapter raises the hoary problem of free will versus determinism of behavior by the organization. Whyte concludes:

We end our discussion of free will versus determinism on a hopeful note. If we undertake to solve human relations problems simply through the manipulation of the direct face-to-face relationship, then we will find that really major improvements are beyond our reach. However, if we learn to deal with the environmental forces that influence the social system, and if we learn to change the pattern of interaction among men, there are tremendous possibilities for improvement ahead of us (p. 32).

Whyte's second chapter is an interesting attempt to set forth the history of the development of his frame of reference. Of particular merit is the short section on the observation of human behavior and its consequences for his interaction frame of reference. At best the author emerges with a description of variables upon which he chooses to focus analytical attention. The last chapter is the best, dealing with feedback of research findings to the organization studied. Here Whyte illustrates the sensitivity to interaction phenomena that makes him one of the best field researchers in American sociology.

Professor Dalton brings to fruition approximately two decades of participant-observer research in six business firms. Only small portions of the research have been previously published. The central theme of the book is that informal human interactions lubricate and give a reality orientation to the formal expectations for work-

ing behavior in the work organization. In elaborating this theme Dalton successively considers power struggles, staff-line relations, operating interpretations of high-level policy, managerial careers, official and unofficial rewards, and some general features of the interconnections between formal and informal action.

The general flavor of the volume can best be captured in two concluding observations by the author dealing with behavioral conformity and non-conformity.

In today's vast systems of rationality the individual conforms as he evades their schemes of detection. Some members find room for personal choice and ingenuity as they strain and thrill in meeting appearances. Others conform to avoid conflict and to maintain the demanded tranquility and uniformity . . . many individual managers and workers do "fight the organization" and there are "individual dynamics" as we saw in the anonymous communications, deliberate misinterpretation of rules for personal, protective, and constructive reasons; the unofficial use of materials and services to reward differential contributions, to cement essential relations; the adaptation of labor contracts; and the "agreements between gentlemen," which allow each "to assume that the other is acting honorably even if he is morally certain that he is not" (p. 270).

We have seen that the problem [of individual freedom] is less one of the individual's being himself than it is one of his being free to show himself as he is. . . . Those who mistake surface conformity in organizations for total conformity and the death of originality, should refocus to concern themselves with the ethics of protective coloration among thinking individuals (p. 272).

Dalton provides rich case materials to illustrate facets of these general conclusions. His work is strikingly parallel to Goffman's social psychology [*The Presentation of Self in Everyday Life*], although the latter's publications are not cited. Beyond the relevance of Dalton's study for organizational analysis lies its significance for the study of social organization. When actual behaviors are studied, as Dalton does, it becomes evident that the containing social system provides imprecise guides for action. For the sociological reader of the study this raises important questions about the functions of social values, institutional norms, and institutional means in ordering actual behavior. The data on social organization in operation make quite clear that the mundane solutions to operating problems, from the individual's standpoint, feed back on structure, norms, and values in ways to modify or even supplant them. The observation of micro-behavior reveals this as no other research technique can.

Dimock's book treats the identical problem faced by Dalton but in quite different terms. Dimock views the essential pathology of formal

organizations as their bureaucratization which destroys individual initiative, originality, and the organization's adaptive ability in its environment. Dimock's approach is to start with outcomes—with organizations that have grown large and prosperous—and to try to discover, from an analysis of their present features, the conditions which have preserved their enterprise against excessive bureaucratization. There results a description of several successful organizations in terms of structure and leadership practices of top management. The general conclusions are that two conditions—decentralization of organization structure, and diffusion of responsibility to appropriate operating levels—are the major safeguards of administrative vitality. There is nothing new in these conclusions. Nor are the particular means used to achieve these results, in the cases examined by Dimock, discoveries that are not already imbedded in the standard literature on industrial management.

It is, perhaps, instructive to ask why there is such disparity in the picture of organization behavior presented by Dalton and Dimock. The answer, I think, is a methodological one. Dalton is interested in process; Dimock in outcomes. Dalton looks at behavior as it is acted out and discovers complexities that evade neat classification into bureaucratic, rule-ordered versus original, problem-solving actions. For Dalton, a bureaucracy has as much rule evasion as rule-ordered behavior imbedded in it. Dimock, on the other hand, takes certain gross criteria of success, such as continuing profitability in the face of vast growth in size, and starting from this outcome, seeks to explain the result by equally gross characterizations of structural features of the organization that have temporally preceded the outcome. Inevitably Dimock must overlook both the present operating behavioral features of the organization studied, and the detailed past history of behavior of its participants. Yet there is already a developed frame of reference and a literature dealing with organization behavior that combines the process and outcome orientations while maintaining the basic contributions of each approach, as the work of Blau, Gouldner, Francis and Stone, March and Simon, and Dubin, among others, demonstrates.

ROBERT DUBIN

University of Oregon

A Neighborhood Finds Itself. By JULIA ABRAHAMSON. New York: Harper & Bros., 1959. xiv, 370 pp. \$5.00.

This is a very detailed account of the efforts of the Hyde Park-Kenwood Community Conference to halt the change in the neighborhood

around the University of Chicago and to stabilize it as an interracial neighborhood. It covers a period of almost ten years, beginning in 1949; it describes the development of the Conference from a small group organized by local churches and citizens, without a budget or staff and without support from local business or the leading institution of the neighborhood—the University of Chicago—to a point where it disposes of a sizeable staff and budget, and has involved great numbers of people in neighborhood stabilization and rehabilitation, and, in more recent years, in a very large-scale urban renewal program.

In its rich detail and scrupulous objectivity the book is unique: no effort is made to exaggerate the effects of the Conference's activities, and perhaps the chief impression one takes away from this book is that of the enormous difficulties involved in reversing the factors that, to use a vague and yet necessary term, lead to the "decline" of a neighborhood. The change in residential composition from higher to lower income groups, the crowding of dwellings, the decline in maintenance, the increase in crime, the over-crowding of schools and parks, all seem an inevitable consequence of the heavy immigration of lower-income groups into a city. The fact that these groups are Negro only speeds up the process. Despite the fact that the Conference had the advice and assistance of some of the country's leading planners and best-known sociologists, the best one can say for its impact is that it probably reduced the rate of change over from white to Negro occupancy and in general slowed down the rate of decline, and that its grass-roots character made it possible for the urban-renewal program to get rather more citizen support (and perhaps even to benefit from citizen suggestion—though this is less clear) than such massive clearance and rebuilding operations generally receive.

These are not negligible accomplishments. But when one reviews this record of intensive and devoted activity by hundreds of skilled people, with the assistance of foundations and leading specialists, one is left with the feeling that perhaps this effort might have been better devoted to some of the more basic factors leading to rapid changes in neighborhoods in Chicago—though conceivably the same energy could not easily have been channelled into programs that did not bear directly on the neighborhood. For example, the author is aware of the central importance of large-scale low-rent housing on vacant land to relieve the pressure exerted on middle- and upper-income neighborhoods by lower-income immigrants to the city. If the energy that went into this

program could have gone into the fight to get the city of Chicago to undertake such a program (as described in the valuable book by Banfield and Meyerson, *Politics, Planning, and the Public Interest*), the long-range results for Chicago might have been better.

In any case, this is an extremely valuable case study in an area which will unquestionably be of great importance for some time to come. One regrets that other cities, and particularly New York, have no studies of the quality of this and the Banfield-Meyerson book.

NATHAN GLAZER

Smith College

Community Organization in Action: Basic Literature and Critical Comments. By ERNEST B. HARPER and ARTHUR DUNHAM. New York: Association Press, 1959. 543 pp. \$7.50.

This volume consists of 75 selections intended to cover the major ideas, programs, and methods in Community Organization within the field of social work. The material is arranged in six sections: The Community and Social Welfare, the Process of Community Organization, Community Organization in Practice, Agencies and Programs, Personnel—Professionals and Laymen, and Community Development in the United States and Elsewhere.

To a substantial extent, the volume meets its objective as a cross-section of the best of the writings in the community organization area (with a perplexingly tiny item from Murray Ross' outstanding book in this field) and in so doing reflects the limitations as well as the contributions of this literature. The contrast is marked between the considerable weighting accorded to descriptions of process and programs, to the actual work of chests and councils, and to presentations of value tenets, as against the sparse attention to analysis, evaluation, research, or underlying theory. This unevenness itself derives, at least in part, from the fact that the field of community organization practice is still in an early stage of theoretical development. Its literature is still heavily concerned with defining scope, objectives, and technique, and is now in a sense trying to catch up conceptually with a rapidly expanded field of operation. The infrequent, meager reference to social science theory (for example, the lack of attention to class and ethnic factors in analysis of community need), however, remains a shortcoming of this volume.

Certain valuable content is included that is not readily available elsewhere. For example, selections are included bearing on two major policy controversies that are current in community organization—the issues of federated

(United Fund) versus "independent" financing in social work agencies, and the merits of separation versus merger of chests and councils. A contribution to a general collection of this kind is the last section, however brief and fragmented, on community development programs, with its major emphasis on such programs in economically underdeveloped countries. Valuable, too, is the provision of several predominantly ideological statements, of importance in the historical development of the Community Organization field.

Certain exceptions may be taken, however, to other aspects of the volume. While the collection is richest in its presentation of the practice of community organization in social work, there is a curious stress, in terms of space allocated, to such subjects as Committees and Recording as compared to the lesser emphasis on Fact Finding, Fund Raising, and particularly Social Action. Granting the pressures of space and the difficult choices to be made, one may still have reservations over the drastic clipping of some of the original texts; the effort at broad inclusiveness may have been overdone at the expense of fuller rendition of the original in the more significant selections.

The most appropriate audience for the volume would appear to be the student, both in undergraduate pre-social work courses and in graduate social work programs. While it will serve as a convenient source book for many practitioners, most of its merit lies in its range of description of practice within social work community organization and its breadth of coverage of points of view as to principles and policies. Thus it makes its most distinct and needed contribution as an orienting and background text in this field.

HERMAN D. STEIN

New York School of Social Work

Origins of Crime: A New Evaluation Of The Cambridge-Somerville Youth Study. By WILLIAM MCCORD and JOAN MCCORD. With IRVING KENNETH ZOLA. New York: Columbia University Press, 1959. xvi, 219 pp. \$6.00.

This volume is based essentially on research data collected in the pioneer "Cambridge-Somerville Delinquency Prevention Project." The ten-year project, which began in 1935, was an effort to control delinquency by providing "friendly, understanding adult leadership" to potentially delinquent youths during their adolescence. A sample of approximately 325 boys were to receive this "treatment." They were compared in the research with a matched control group of approximately the same size. In 1948 an analysis by Edwin Powers and Helen Witmer, *An Experiment in the Prevention of*

Delinquency, revealed "discouraging results." The analysis of court and police records showed that the experimental group committed about the same number of offenses as the control group.

The McCord study, utilizing the extensive case records of the project, focuses on two basic issues: (1) Did the treatment help prevent criminality? (2) What social factors operating on this group contributed to later criminality?

The analysis proved (with minor variations) that the treated youths committed as many official acts of delinquency and later crime as the control group. Also, no distinct differences were found between the samples with reference to type of offense or age at which offenses occurred. The conclusion of the McCords on the issue of "treatment effectiveness" is in part a further validation of the original Powers and Witmer analysis.

The McCords make a contribution to a better understanding of the treatment used in the project by an extended analysis of many crucial aspects of the approach, including contact frequency and intensity (Chapter 3, "The Effects of the Treatment Approach"). Comparing the control and treatment groups, they further examine such factors as number of convictions, types of crime, recidivism, and age relationships.

Although other data and analyses are relevant, the central thesis of the volume is presented in Chapter 4, "The Origins of Crime." In this section the researchers examine such factors in the background of criminality as I.Q., neighborhood conditions, religion, parental discipline, "home atmosphere," and father-mother roles. Much current delinquency research involves efforts to explore past factors (e.g., discipline, parent-child affection, etc.) through "recall interviewing." One basic strength of the McCord analysis is that the data were not "worked backward," or as they stated it, "retrospective bias has been eliminated."

The core of the research findings is clearly stated in Chapter 8, "Summary of the Causes of Crime." Although the findings of the study about causation are not shockingly new, the McCords' conclusions tend to lend further empirical validation to some currently popular concepts of causation and to suggest the re-examination of other notions. As they report:

This research has demonstrated—at least, for the boys in our sample—the falsity of several popular theories concerning the origins of criminality.

1. We did not find a strong, direct relationship between criminality and residence in a slum neighborhood.

2. We did not find a direct relationship between criminality and disciplinary methods except when a child is rejected by his parents or has deviant parental role models.

3. We did not find that punitive, harsh discipline (in whatever form administered) prevents criminality. Under certain conditions, consistently punitive discipline may deter criminality, but erratically punitive discipline promotes it.

4. We did not find that sons of passive males turn frequently to criminality to assert their "masculinity."

5. We did not find that broken homes constitute the type of atmosphere most conducive to criminality (p. 172).

In Chapter 9, "Implications for the Future," the authors illustrate another current problem in the field of delinquency prevention: the search for a simple solution to complex phenomena. The authors describe an instance in which the head of a school guidance department in a small town attended a lecture on the research findings. The guidance counselor sought ammunition to expand his counseling operation, hoping to prove to his Board of Directors that "counseling could prevent delinquency." He left disappointed, since he could not in clear conscience request funds, at least on the grounds of this scientific research evidence. The McCords' volume maintains a scientific integrity which does not compromise itself to the quick and easy solutions often demanded by contemporary laymen and practitioners working at "delinquency prevention."

Although few new and scientifically conclusive statements about the "origins of crime" are to be found in this volume, the research, its straightforward presentation, and theoretical logic make for a significant and valuable contribution to "the search for causes."

LEWIS YABLONSKY

University of Massachusetts

Mental Health and Manpower Trends. By

GEORGE W. ALBEE. Monograph Series No. 3. Joint Commission on Mental Illness and Health. A Report to the Staff Director Jack E. Ewalt, 1959. New York: Basic Books, 1959. xliii, 361 pp. \$6.75.

This study of personnel resources and manpower needs points up the current shortage of qualified physicians, psychologists, nurses, and social workers in public mental hospitals and clinics. These present-day inadequacies in staff are inconsequential, however, in comparison with the manpower deficiencies that can be anticipated in future years. This book is not a propagandistic effort achieved by the selective reporting of data. Albee is objective, thorough, and careful in his analysis and has taken into account the problems incidental to pooling data from various secondary sources.

In part, the shortages of qualified personnel are related to low salaries and the high costs of professional education. Albee's separate analyses

of manpower prospects in psychiatry, psychology, social work, and psychiatric nursing contradict any notion, however, that personnel deficiencies are solely a financial problem. He points out, for example, that the shortage of social workers is connected with college students' unawareness of career opportunities in this field. Medical school graduates, on the other hand, are lured away from psychiatry to other branches of medicine because of its reputation as a low status specialty, as well as the length of time required to complete training, particularly for psychoanalysts.

Albee's analyses of the selection, recruitment, and training of various professional groups draw on a variety of sources. There are, however, a number of relevant sociological investigations that he did not consider, particularly recent studies of occupational choice among physicians and nurses. On the other hand, his analyses do raise a variety of questions for sociological research. For example, does the large number of foreign physicians filling residencies in certain state mental hospitals modify physician-staff and physician-patient relationships in these institutions?

In various ways, Albee has provided a context for his data, and thus the book offers more than a competent analysis of present conditions and future needs. He considers the mental health personnel situation in terms of the general problem of medical education, the need for trained personnel in other fields, and the growing shortage of college instructors. Albee, of course, recommends the training of mental health personnel at as rapid a pace as possible. His major plea, however, is not for a crash program to produce mental health specialists, but for a modified educational system that can cope effectively with the shortage of technical and professional personnel that prevails in so many fields.

HOWARD E. FREEMAN

Harvard University

The Psychology of Affiliation: Experimental Studies of the Sources of Gregariousness. By STANLEY SCHACHTER. Stanford: Stanford University Press, 1959. 141 pp. \$3.75.

Does misery love company? Frame this query as a research hypothesis and you have the starting point of Stanley Schachter's fascinating experimental study of affiliative tendencies.

Drawing on accounts of consequences of social isolation, the author formulated the notion that if conditions of isolation produce anxiety, perhaps conditions of anxiety lead to increased desire for social ties. In Schachter's neat and simple test of this proposition, college girls

participated in an experiment ostensibly concerned with the effects of electrical shock. Some subjects were placed in high-anxiety conditions—"These shocks will hurt, they will be painful"—while others were given a much milder perspective. (Schachter reports convincing evidence that "high-anxiety" conditions did indeed lead to anxious feelings. This information, reassuring us that naive subjects still are stockpiled on our campuses, is, I suppose, at once surprising and gratifying to almost any experimentally-oriented social scientist.) After manipulating anxiety, the investigator asked each subject whether she preferred waiting her turn alone or in the company of others. Responses to this query gave clear evidence that affiliative desires increase with anxiety.

This tidy result is merely a point of departure; Schachter directs the bulk of his efforts toward clarifying and elaborating the nature of this relationship between anxiety and affiliation. And the systematic manner in which he devises experiments and draws on research and observations of others to construct a stable evidential structure is impressive.

Additional experiments reported in the monograph lead to the following conclusions: (1) Given a choice, individuals facing an anxiety-provoking situation prefer to be with others in a similar plight, rather than to be alone or with others facing a different set of circumstances. (Not considered, however, is the likelihood that some might prefer the company of a person with special technical competence to meet the needs of the fearful situation, rather than that of others simply riding in the same anxious boat.) (2) The relation between anxiety and affiliation is independent of the opportunity to communicate. (3) In fearful circumstances, first-born and only children are more anxious than later-born; and with anxiety held constant, those earlier in the birth order show stronger affiliative desires. (4) Gregariousness is positively related to hunger, as well as to anxiety. (5) Two factors contributing to an explanation of the anxiety-affiliation relationship are anxiety reduction and self-evaluation.

Schachter is at his best following the trail leading to an understanding of the effects of ordinal position on the affiliative response to anxiety. Here, using a technique one wishes the author might have resorted to more frequently, he buttresses his experimental results with counterpart data from real-life situations and draws these findings together by means of an assumed relationship between ordinal position and dependence.

A major contribution of this volume may lie in the formulation that "emotions or feelings . . . require social evaluation when the emotion-

producing situation is ambiguous or uninterpretable in terms of past experience." This extension of the theory of social comparison processes, as Schachter points out, may serve to integrate our ideas of social determinants of emotional states in a common conceptual scheme with social determinants of opinions and abilities.

This volume is a superior piece of work, presenting shrewd research planning and careful thinking in a sound package. It will doubtless stimulate further exploration of the factors behind man's social yearnings.

JOHN D. CAMPBELL

National Institute of Mental Health

Personality and Persuasibility. Edited by CARL HOVLAND and IRVING JANIS. Yale Studies in Attitude and Communication, Vol. 2. New Haven: Yale University Press, 1959. xiv, 333 pp. \$5.00.

This book is a description and evaluation of a series of studies aimed at determining whether or not there are factors associated with personality which determine the degree of persuasibility of the individual independently of subject matter, the source of appeal, or the mode of communication of any particular persuasive communication. If such "unbound factors" exist, the authors argue that they should be detectable by one of two methods: (1) The method of "systematic variation" in which the subject would be exposed to "a large cross-sectional sample of social communications representing a wide range of topics, appeals, arguments, communications characteristics, media characteristics, and factual context." In this case, one would then attempt to separate the variance in responses attributable to these bound factors from unbound factors representing individual general persuasibility. (2) The "exclusion method," in which the subject is presented with "stripped-down communications" from which "everything has been excluded except the source and conclusion."

The bulk of the book is devoted to recounting a series of experiments conducted by the authors and their associates using both of these methods. These researches indicate "that there is such a factor as general persuasibility, although there are certain limitations to its generality imposed by the experimental procedures employed. There is evidence that persuasibility exists as a 'content-free' factor; that is, it exists independently of the subject matter presented in any particular persuasive communication."

Some of these studies showed consistent individual differences in persuasibilities and more or less correlation between persuasibility and

such factors as self-esteem, sex, hostility and aggressiveness, neurotic defensiveness, intelligence, and independence.

The authors indicate that the research techniques are not as good as they would like, and, in a chapter on summary and implications, they point out the need for further research; they seem especially disappointed because they were able to introduce relatively little systematic variation in communication media and less than an ideal amount of variation in source. However, they did achieve considerable variation in content. They also suggest that, since the studies were done mostly on a narrow range of social and demographic variation, it would be desirable to broaden the subject variety. At this stage of their findings, it appears that personality factors "play only a very minor role as determinants of persuasibility." However, they argue—and cogently—that, given the limits in sample and experimental technique, it is really too early to arrive at any definitive conclusions.

In order to make organized sense of the material they have accumulated, they propose a theory for describing the psychological processes which mediate the effectiveness of persuasive communication. This theory is based on the three concepts of attention, comprehension, and acceptance. The individual differences in the degree to which these three factors operate give rise to different degrees of susceptibility to persuasion. They argue that such individual differences are attributable mainly to two types of personality factors: "(1) Ability factors, which are determined primarily by intellectual factors and training; and (2) motive factors, which are determined by temperament, conscious goal strivings, unconscious impulses, and habitual mannerisms of defense."

Two appendices give the materials used for testing persuasibility and, when used in conjunction with the chapters recounting the experiments, should provide the reader with the basis for further research in this intriguing and potentially important field.

DONALD N. MICHAEL

Dunlap and Associates

Assessment of Human Motives. Edited by GARDNER LINDZEY. New York: Rinehart & Co., Inc., 1958. 273 pp. \$5.00.

FIRO: A Three Dimensional Theory of Interpersonal Behavior. By WILLIAM C. SCHUTZ. New York: Rinehart & Co., Inc., 1958. 267 pp. No price indicated.

Assessment of Human Motives consists of a collection of papers by a number of distinguished psychologists who were participants in a con-

ference at Syracuse University. Although the conference presumably had as its focus the problems connected with the assessment of motivation, apart from Lindzey's introductory chapter, most of the papers are theoretical essays showing little in the way of detailed concern with measurement problems. Most of the authors have elsewhere, in separate publication, presented a more satisfying, fuller offering of their points of view. The varied assortment does, however, enable the reader to obtain an overview of what is "cooking" in the field of personality psychology.

Several over-all impressions stand out. First, it is evident that psychoanalytic theory continues to be the dominating framework for much of the thinking and research. However, there is a clear and insistent call for applying more rigorous intellectual standards to the formulation of psychoanalytic theory and to the collection of data to test psychoanalytic hypotheses. Secondly, there is evidence of a marked shift from an impulse or drive-oriented conception of motivation to an emphasis on its cognitive determinants. To oversimplify it somewhat, the "new look" of perceptual psychology is reversed: instead of our needs determining our perceptions and cognitions, our cognitions determine our needs. A third impression reflects a lack of emphasis in the papers. Despite the vast outpouring of writing on "personality and culture" in recent years, the essential conception of personality presented in the various papers is that of the isolated individual rather than that of the individual in society.

In the individual papers, Lindzey raises some major methodological issues and heroically attempts to provide an organizing focus for the various papers by a series of questions centering about these issues. His questions—perhaps too general—were quickly brushed off by most of the contributors. Kelly ("Man's construction of his alternatives") repudiates the notion of motivation and, by sleight of hand, replaces it with the notion of anticipation. Festinger ("The motivating effect of cognitive dissonance") provides an excellent summary of his theory of cognitive dissonance and some ingenious data bearing upon it. Klein ("Cognitive control and motivation") gives the most sophisticated, research-phrased statement of psychoanalytic ego psychology that I have seen in print. Schafer ("Regression in the service of the ego") is concerned with the personality conditions which foster creativity. Unfortunately, the conditions he cites appear to be merely apt characterizations of psychological health and psychopathology. Janis ("The psychoanalytic interview as an observational method") indicates how he believes

the psychoanalytic interview can be used to provide scientific data of critical importance to social-psychological as well as to personality theory. I envy Cattell's ("The dynamic calculus") energy and admire his resourcefulness in employing a wide variety of measurement procedures but I find myself unable to take seriously the results of his factor analyses or of his mathematical formulations so long as he does not appear to take seriously the limitations that the scales of measurement involved in his data place upon their statistical treatment and mathematical analysis. Murray ("Drive, time, strategy, measurement and our way of life") and Allport ("What units shall we employ?") each provide, in different ways, an over-all perspective of the domain to be covered by the personality theorists.

FIRO is an irritating book. The author and its publisher have done themselves a disservice by publishing it in its present form. It is pompous, overstated, and oversold. The book's jacket includes such slogans as "new approach to behavioral science," "group behavior explained," and "birds of a feather produce together." None of these statements is warranted by the contents of the book.

Unfortunately, many serious readers will, I believe, be sufficiently alienated by the style so as to be unable to go beyond the introductory chapters. Although there are obvious inadequacies in Schutz's theoretical formulations and frequently the data he cites in support of his theorems are of questionable relevance and occasionally his statistical analysis of his data is dubious, the book has some merit. Schutz has focused his theory and research on variables which are clearly of importance to the understanding of interpersonal phenomena. He describes three fundamental interpersonal needs: a need for inclusion, a need for control or power, and a need for affection or closeness. With respect to each need, any individual has some level or quantity he wishes to express toward others and some level he wishes to receive from others. A questionnaire technique to measure each of the three needs, in the two forms of "expressing" and "wanting" is described. Although he offers some notions about the development of the interpersonal needs (presumably the "needed levels" are set for the individual by his interactions with his parents), the major portion of the book has to do with interpersonal compatibility. Research by Schutz or his colleagues is cited which indicates a relationship between compatibility and various sociometric measures and a relationship between compatibility and group productivity. His findings are reasonable and interesting even when they are not necessary consequences

of his assumptions. His discussion of interpersonal relations and of group functioning is often insightful and is suggestive of fruitful possibilities for future research. The various assets of his book should not be overlooked simply because the book claims much too much.

MORTON DEUTSCH

Bell Telephone Laboratories

Social Mechanisms: Studies in Sociological Theory. By GEORG KARLSSON. Glencoe, Ill.: Free Press, 1958. 156 pp. \$5.00.

The bulk of Karlsson's work in this book is an overview of psycho-sociological models, usually of the more rigorous, formal variety, accompanied by a summary of empirical findings which bear upon aspects of the social mechanisms involved in these models. The three chapters are concerned with social diffusion, group membership, and general group interaction. In the main, Karlsson tends to juxtapose the models one after the other—and then to write summaries of the empirical literature from his notes. His diagrammatic summary (p. 141) of the empirical interrelations discovered among some seventeen variable clusters with respect to group interaction is exemplary as a way of capsulating a large mass of material. These compilations are well worth the purchase price of the book—and will help broaden the scope of many theory courses in the next few years ahead.

Although Karlsson is an Uppsala sociologist, he is not bound by Segerstedt's focus on norms. Karlsson's eclectic mode yields a mature but highly general integration of interaction theory of his own. In his introduction he sketches his conceptual scheme (pp. 11–16), and in his concluding remarks (pp. 134–139) he renders a slightly more formalized presentation. The clarity with which Karlsson writes is illustrated in this summary of his general interaction model:

... the group product depends upon what the group members do, and what the members do depends upon their motivation, which in turn depends upon how they perceive the situation in general, what has happened in the group and what they expect to happen, and finally the results that are expected to come out of the whole process. The perception of the situation will depend upon the actual situation and what the person has been told. The perceived plans of the group members will depend upon what is known of their value scales, what they have been doing, and the perceived situation. The value scales, finally, are influenced by social pressure from other persons.

Three efforts toward the "further developments of model building" serve as conclusions to each of the three chapters. His work on a

"Model for Pure Interpersonal Communication" and the "Model for Combined Mass Media and Interpersonal Communication" at the end of Chapter I is provocative. Especially useful is its abbreviated illustration of the use of Monte Carlo computations when sociological complexity defies formal mathematization. But the suggestions made for model development in the other chapters are less interesting. They fail to integrate the models into super-models—and they largely leave unbridged the "considerable dissimilarity between . . . theory and data" (p. 16). Karlsson's reluctance to justify his suggestions by revealing his underlying reasoning is particularly vexing, for his book abounds with insights "in the small," which make it a delight to read. The stigmata of the high quality of Scandinavian social science characterize Karlsson's work.

HAROLD GUETZKOW

Northwestern University

Experiment Perilous: Physicians and Patients Facing the Unknown. By RENÉE C. FOX. Glencoe, Ill.: Free Press, 1959. 262 pp. \$5.00.

Experiment Perilous is a sociological study dealing with the interrelationships of the physicians and patients who composed Ward F2, a metabolic research ward of fifteen beds in a large city hospital. The Ward had a distinctive flavor because the patients in it had a variety of metabolic, endocrinological, cardio-vascular, renal and malignant conditions from which they were not likely to recover completely, due in part at least to the limited medical knowledge about these diseases. Consequently, the approach of the physicians on this Ward was frankly experimental as they attempted to study the actions of ACTH, cortisone, and various other adrenal cortical steroids upon the patients and to record their reactions to these bio-chemicals. In this atmosphere of combined uncertainty—of the patients with respect to their diseases, and of the doctors with respect to their inability to help them—Professor Fox attempts to depict the kind of social system the patients and doctors evolved.

In addition to participant observation Professor Fox utilized other techniques in her research, such as formal interviews with physicians and patients, analysis of hospital charts, examination of a vast file of letters written to and received from patients, journalistic accounts of the patients' diseases, and numerous papers concerning the experiments on the patients reported by the physicians in their scientific journals. Professor Fox's project was part of a larger research undertaking that attempted to investigate the

long-term bio-chemical and psychological effects of various kinds of stress upon man. Eschewing any kind of statistical analysis other than a brief count of the age and education of the patients in Ward F2, Professor Fox has succeeded in conceptualizing the social world of this type of ward so that it rings fairly true and squares with numerous other accounts in the literature of how men organize to handle those stresses characteristic of such situations as the battle-field, prison, concentration camp, and the like.

Her central chapters, four in number, deal with the problems and stresses of the physicians and the patients and how both physicians and patients came to handle the particular problems and stresses with which they were confronted. With respect to the physicians, Professor Fox succeeds most admirably in conceptualizing their uncertainty in the face of the unknown diseases, the limitations of their therapies, their attempts to motivate the patients to serve as research subjects, and the conflicts inherent in a situation involving research objectives and therapeutic goals. She shows how the physicians through group morale, humor, games of chance, and the development of more personal relationships with their patients came to terms with the stresses to which they were subjected. A major interest here is her description of how the physicians tend to resort to magical devices as they realize the limitations of their logical and rational procedures for diagnosing and treating their patients. Much the same procedure is followed in dealing with the patients. The patients, from a slightly different perspective, were also faced with the problem of uncertainty but in addition they were plagued by the changes in their activities on being hospitalized, their inability to see therapeutic progress, and the constant search for meaning in their lives. To resolve their stresses, the patients relied on humor, magic, religion, and medical "expertise." As the physicians got their satisfactions from the publication of their research results, both positive and negative, some of the patients got their satisfactions by attaining "medical stardom" and seeing their cases and diseases written up in the popular press for mass consumption.

Any student who thinks that the method of participant observation is a valuable tool in social research should not neglect Professor Fox's chapter giving a step-by-step account of her research procedures from the moment she was introduced to the Ward to the time when the pattern of life in the Ward began to take shape from her recorded observations. Both in a personal and professional vein, Miss Fox describes carefully her own integration into the

life of the Ward and her gradual realization that she had become socialized with respect to its values, uncertainties, and expectations. Perhaps, and Professor Fox makes the point, it may be impossible to portray in any significant fashion such a small social system unless one becomes an actual part of it; she not only observed but took part in the activities of patients and physicians that characterized the daily round of Ward life.

Her final chapter, in which she attempts to deal with the significance of this study, is perhaps the most disappointing because it never succeeds in going beyond a mere summary of the findings even though she does try to show that her generalizations have an extension beyond this particular hospital situation. Like Caudill's study of a psychiatric ward, Professor Fox's study portrays the forms, values, practices, and expectations of a particular social system that, in a sense, current sociological theory would enable us to predict. We know these things will be there, even though we cannot predict their exact content; and of course it is content material that makes a research account like this one most interesting.

While accounts like Professor Fox's enable us to rationalize more accurately a given social situation we are left frequently in the dark as to how such knowledge can be utilized effectively to improve the common lot of patients and physicians and cause the ward social system to function more satisfactorily for all. Perhaps this will never be the case, and in a broad general sense the study achieves a larger goal by contributing to our humanity and education. Perhaps our concern is somewhat typified by the way an old F2'er introduced Miss Fox to a new patient: "This is Miss Fox. She's not a doctor exactly. She's not a patient exactly. But she falls somewhere in between." While this statement appears to be a significant one for describing a successful participant observer, let us hope that sociology in its theoretical and empirical advances of the next decade can work itself out of its too often "in between" condition.

H. WARREN DUNHAM

Wayne State University

Alcohol in Italian Culture: Food and Wine in Relation to Sobriety among Italians and Italian Americans. By GEORGIO LOLL, EMIDIO SERIANNI, GRACE M. GOLDER, AND PIERPAOLO LUZZATTO-FEGIZ. Glencoe, Ill.: Free Press; Publications Division, Yale Center of Alcohol Studies, 1959, xv, 140 pp. \$4.00.

This book results from research undertaken jointly by the Yale Plan Clinic (a division of

the Yale University Laboratory of Applied Biodynamics and Center of Alcohol Studies) and the Istituto di Alimentazione e Dietologia in Rome. It is the second Yale Center monograph to be concerned with research on non-problem drinking. The first, on alcohol and the Jews, related the rarity of Jewish alcoholics to social and cultural traditions. The studies of alcohol in Italian culture conclude on a similar note.

Several studies are reported. The first compares a group of 247 Italians in Rome with a group of 251 Italian-Americans comparable in age, sex, education, religion, economic status, and physical and emotional health. Subjects, studied for periods varying from several weeks to several months, each prepared a dietary diary, and were given physical, sociological, and psychological examinations as well as a sugar tolerance test. The book reports considerable information concerning the characteristics of the sample and the habits of eating and fluid consumption.

The second part of the book reports research, previously published, concerning the consumption of wine and milk and the attitudes of Italians towards these liquids. The data were collected in 1954 in Italy on a stratified sample of 1,453 adults.

Although the per capita use of alcohol is substantially larger in Italy than in the United States the rate of Italian alcoholism is approximately one-eighth that in this country. Practically all the Italian cases reported, but only one quarter of the American cases, were accompanied by nutritional disorders. With reference to milk, the Italian attitude seems to be that it is fine for children (over 85 per cent of the children are breast fed). Since milk is not drunk by adults, the authors conclude that the relative sobriety of Italians does not result from the adult consumption of milk (nor to the lack). The extensive use of milk by children, followed in adulthood by the consumption of wine with meals, is considered an important factor in explaining the comparatively low inebriation rate among Italians. The prevalent Italian attitude that the drinking of wine cannot lead to alcoholism may be another significant factor. These facts, plus the Italian tendency to regard drinking as a necessary part of eating, lead the authors to conclude that dietary habits and cultural traditions may explain the relative sobriety of Italians. Significantly, the rate of alcoholism of Italian-Americans approaches that of Americans in general as the first, second, and third generations begin to adopt the mores of the new country, drinking more and more hard liquor and drinking between meals.

These studies are valuable in that they provide

preliminary evidence concerning the importance of social and cultural variables in the dietary habits of groups and suggest the need and direction of further research. Certainly drunkenness is a major social pathology of the day, and one requiring fundamental scientific data such as reported here. The authors, wisely, do not attempt to generalize beyond the data in an attempt to provide answers to some tantalizing questions. For instance, if the attitudes and dietary habits of the Italians are as reported, why are they different from those, say, of Americans? If the answer lies in some kind of cultural tradition, why have different drinking traditions arisen among different groups? Though these questions remain unsolved, a substantial groundwork for their solution is being provided by the patient accumulation of research results such as those reported in this small volume.

LUIGI PETRULLO

Office of Naval Research

Continuity and Change in African Cultures.

Edited by WILLIAM R. BASCOM and MELVILLE J. HERSKOVITS. Chicago: University of Chicago Press, 1959. vii, 309 pp. \$7.00.

For over thirty years the Department of Anthropology of Northwestern University has pioneered African studies in the United States. This volume, consisting of fifteen essays by former students of that Department, is an effective tribute to the work of Melville Herskovits, Director of the African program. Originating as it did with an interest in the Negro of the New World, it was perhaps inevitable that this work should have an historical bent, that it should concentrate particularly on the ways in which cultures change. In their introduction, the editors maintain that this symposium, as compared with similar anthologies on African societies, introduces the dimension of time (or history) and deals with "culture" rather than "social institutions." The "other studies" referred to are those carried out by those social anthropologists who have worked in the sociological tradition which derives from the work of Durkheim, Weber, and others. As compared with such studies—and one might mention Evans-Pritchard and Fortes' *African Political Systems* as an example—Herskovits and Bascom have given us a volume which is certainly wider in scope. Following a general introduction, there are three papers covering the languages, art, and music of Africa. Greenberg provides an excellent summary of his important contributions to the classification of African languages. And while the present state of knowledge does not permit a similarly comprehensive picture of music or

art, Merriam has rendered a useful service in presenting a critical review of writings on African music.

Apart from a study of African polygyny, the remaining essays are studies of particular features of particular societies, such as religion, economy, marriage, lineage structure, and the position of women (yes, by a woman). All of them touch in some way upon problems of social change, or, as the authors prefer to have it, "acculturation." These papers have been considerably influenced by the work of social anthropologists. Indeed Lystad's analysis of differences in the changing marriage systems of the Agni and the Ashanti resembles in several ways Fortes' (unmentioned) study of two Ashanti villages, "Time and Social Structure." A comparison of the two studies suggests a modification of the editor's claim that these essays are original in introducing the time or historical dimension to African anthropology. In the first place, if social anthropologists have tended to concentrate on the study of a synchronic situation there are certain sound technical reasons for so doing. In the second place, there have been other attempts to introduce the time dimension on a level which, though more restricted, may well have more significant implications for the study of social action. Nor does the other theoretical point of the editors, the concentration on "culture" as against "social institutions," appear to have contributed greatly to the bulk of the essays, apart from widening the range of the symposium itself. The reason for this is deep-seated. Many sociologists and anthropologists in this country have come to regard their disciplines as having distinct frames of reference and even discrete bodies of data. Theoretical schemes have been elaborated giving the "cultural system" equal status with the "social system," and these areas are allocated to different specialists who tend to become involved in maintaining the independence of their fields of study. A glance at contributions to this volume suggests that this dichotomy could do with "an agonizing reappraisal." The best of the analyses of specific societies in this volume make use of the work of social anthropologists and in effect ignore the dichotomy which the editors seek to impose. Consequently, if this volume fails to confirm altogether the editors' standpoint, it succeeds not only in reviewing areas such as linguistics (which other symposia do not touch) but also in emphasizing that students of society, whatever the declared views, often tend in their empirical work to move in converging directions.

JACK GOODY

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Organization of Work: A Comparative Analysis of Production among Non-Industrial Peoples. By STANLEY H. UDY, JR. With an Introduction by GEORGE P. MURDOCK. New Haven, Conn.: HRAF Press, 1959. xiv, 182 pp. \$3.95.

At the beginning of his book Dr. Udy states his objectives in a modest and straightforward manner. "All production organizations exist within some society, and may further prove to be contained by some subgroup within a society. In this sense, all production organizations, technological processes, and reward systems function within a social setting. . . . the study seeks to discover the principal ways in which technology, production organization, reward systems, and social setting are interrelated." The program is ambitious but it makes sense.

In Chapter 8, Dr. Udy summarizes his conclusions. These, in the main, consist of 58 numbered propositions which he claims to have verified. Two characteristic specimens are:

27 Familial organizations tend to be basic-auxiliary, diffuse and permanent.

51 *Bolhon* type organizations tend to involve unbalanced leiturgical systems relative to *palihog* types, which are relatively more likely to involve balanced compensatory systems.

The elaborate coded jargon which Dr. Udy has devised for the presentation of his argument is in itself an effective screen between author and reader but those who are driven by duty or curiosity to pursue the matter further must face the added obfuscation of a maze of probability coefficients. For example, when proposition 27, cited above, first appears at p. 60, it is supported by the coefficients: $Q = +.99$, $\chi^2 = 97.23$, $P < .001$. This implies, I take it, that probably it is so! But what is so? What does proposition 27 mean in plain English? There is no index or glossary of special terms but the reader may perhaps remember that *basic-auxiliary*, *permanent*, and *diffuse* have been defined at pp. 42-44. These definitions seem to reduce proposition 27 to the profound statement that: "In many societies kinsmen can be observed to help one another on some jobs part of the time with no obvious objective." What an amazing discovery! As for the leiturgical *bolhons* and *palihogs* of proposition 51, an hour's hard work still leaves me baffled. As I understand it, Dr. Udy is here referring to the well-known phenomenon that a stag party where everyone pays for his own drinks is less expensive than one at which each man stands a round of drinks to all the others. Dr. Udy seems to be disturbed by the frequency of reciprocities of this latter kind, which, on his analysis, will bring material rewards to the organizer of a productive unit but nothing to those who are organized. He ex-

presses anxiety lest the absence of contractual organizations among peasant societies may imply that these societies will have difficulty in converting themselves into a more modern form. Herbert Spencer, I recollect, argued on the same lines.

In short, I value the content of this book as nil; all Dr. Udy's findings seem to me either self evident or meaningless. The methodology, however, deserves further comment.

The 58 verified propositions imply that the author claims to have discovered sociological laws which are true either of human society as a whole or else of certain types of social unit selected by himself. These "laws" are represented as true in a statistical, not absolute, sense. Certain kinds of correlation between technology and reward system, etc. "tend to occur." This must mean that sometimes they occur and sometimes they don't, so that in any particular, previously unexamined, instance the position is quite open. Assuming that the statistics are properly based on objective fact, one may surely ask: Of what possible use could such information be to anyone in any circumstances? But are the facts correct at all? Are they even the kind of fact about which such "tendency" statements could sensibly be made?

Dr. Udy approaches anthropological materials with the assumptions of the school which C. Wright Mills has labelled "abstracted empiricism." He assumes that human societies can be treated as the units of a statistical population and that statistical analysis of the facts reported about individual units will lead to the discovery of facts which are true of the population as a whole—in this case, human society as a whole.

Two questions are therefore crucial. Are the societies from which Dr. Udy's facts are drawn comparable as units of a statistical population? Are the facts recorded of these societies true in a sense which makes them appropriate subjects for statistical quantification?

The evidence which is alleged to give statistical support to Dr. Udy's 58 numbered propositions consists of information concerning 426 "clearly reported production organizations" as docketed in the Yale Human Relations Area Files. The 426 production organizations are distributed among 150 societies which are said to constitute a "carefully selected representative sample of nonindustrial societies." The basic statistical sample used to verify the various numbered propositions comprises one production organization from each of the 150 societies. There is plainly a fundamental assumption that the record concerning any one production organization and any one society is comparable in quality and kind to the record from any other.

The complete absurdity of this assumption is readily demonstrated.

One of the listed societies is Tikopia (four productive organizations). Tikopia has a population of about 1,800 living in a territory of three square miles. The economic arrangements on this island have been recorded with meticulous detail by one of the world's foremost anthropologists. Another sample society is the Chinese, population 650 million odd, (four productive organizations). Evidence is drawn from four small villages (three in Yunnan and one on the Lower Yangtze), Morton Fried's *Fabric of Chinese Society*, and a popular work called *China: the Land and the People*. It surely does not require a great deal of ethnographic experience to realize that the facts which are alleged to be true of the Tikopia are likely to be true in some quite different sense from facts alleged to be true of the Chinese as a whole. A third society is the Burmese, population about nine million (four productive organizations). In this case the main ethnographic source is the autobiography of the English-educated daughter of a Burmese official of Mon race. She was under 30 when she wrote her book and had already spent at least eight years of her life outside Burma. The only other evidence cited for the Burmese consists of four pages of a case report on a single family of fishermen refugees. A fourth society, the Yami, has a population of 1,400 (one productive organization). Here all the evidence comes from one page of an unpublished lecture by a Japanese linguist who last visited the Yami over 20 years ago.

Are we really to believe that in assessing the socio-economic tendencies of the human race the Tikopia and the Yami count equal to the Chinese and the Burmese or that the fat detailed volumes of Professor Raymond Firth are to be assessed as no more significant than the casual recollections of Dr. Erin Asai?

Not only is there no discrimination between good sources and bad sources or between large and small societies, but bad sources are often preferred to good. Examples: the Tiv are on the list but not the writings of Professor and Mrs. Paul Bohannan; the Iban are in the list but the only source is a grossly inaccurate missionary account published in 1911; Dr. Derek Freeman's brilliant up-to-date ethnography is ignored.

I have mentioned these particular cases because they happen to be societies of which I have special knowledge, but I suspect that similar criticism might be levelled against at least half the evidence in the file.

The whole schema rests on the assumption that the population of the world can usefully be divided up into a limited number of mutually

exclusive named societies and that, if this is done, the cultures of these different societies can be discriminated by drawing up a long list of ethnographic facts and noting whether each listed item occurs or does not occur among the reported customary behaviors of each of the peoples concerned. But the Chinese and the Burmese and the Iban and the rest do not adhere to set customary forms in this way and any kind of analysis which assumes that they do is worthless *ab initio*.

All this adds up to an extremely harsh judgement of what has clearly been a most painstaking piece of work and it is only right to mention that the dust jacket carries the approving comments of a number of highly respected American scholars. I am sorry that I cannot share their enthusiasm.

E. R. LEACH

Cambridge University

The Remaking of a Culture: Life and Education in Puerto Rico. By THEODORE BRAMELD. With the assistance of ONA K. BRAMELD and DOMINGO ROSADO. Foreword by OSCAR E. PORRATA. New York: Harper & Bros., 1959. xviii, 478 pp. \$7.50.

During the past decade the Puerto Rican government's annual contributions to public education have hovered around one-third of the total budget. A part of this amount goes each year to support research activities conducted under the auspices of the Centro de Investigaciones Sociales of the University of Puerto Rico. Expanding education and extensive research into virtually all aspects of Puerto Rican culture are thus two of the chief devices by which the government hopes to prepare itself and the populace to meet the challenge and the consequences of Operation Bootstrap and its god-child, Operation Serenity.

It is no wonder, then, that they should have welcomed a project like the one detailed in the present book—a project which offered to wed cultural research to a concrete program of education.

Professor Brameld's work is in large part the product of a series of intensive interviews with a selected group of twenty "grassroots" respondents from two of the communities dealt with in Julian Steward's *The People of Puerto Rico* and from a large urban center; in addition, sixteen "national leaders" were chosen from a panel prepared by leading Island educators. The "guiding questions" in these somewhat loosely structured interviews dealt with the major categories of "cultural order, process, and goals." But since the study was primarily qualitative, Brameld formulates his conclusions

from the data in the framework of what he calls the "key principle of social consensus," which includes, among other factors, "maximum agreement among those who have considered and communicated the evidence" and "action . . . to test the agreement obtained" (p. 430). In addition to the interviews, Brameld made extensive use of published and unpublished research materials (chiefly the Steward study) in formulating his guarded generalizations about Puerto Rican values, family, class structure, education, national character and modal personality, religion, etc.

It is Brameld's belief that formal education may be used "not merely to transmit but to modify and reconstruct outmoded arrangements" (p. 19); that "education . . . on this Island is both actually and potentially a primary cultural force" (p. 393). And as a major corollary of this belief he asserts that a detailed knowledge of the culture within which the educational system operates is a necessary prelude to the implementation of an action-education program—a theme which he has stressed in some detail in an earlier work, *Cultural Foundations of Education*.

Professor Brameld has done a fine job of synthesizing the findings of other research workers and relating these to the mixed data from his own set of respondents. His frank admission of philosophic and applied bias is as admirable as his detailed anticipation of objections to method, result, and interpretations is disarming.

But after all the apologies are recorded the reader is still justified in raising several questions about method and conclusions. For example: (1) How, in the absence of quantitative devices, did the author extract his "social consensus" from the mass of differing and often contradictory responses—especially in the tenuous realms of valuation, "metaculture," national character, and the like (cf. p. 31 and p. 430)? (2) Isn't it possible that Brameld shrugs off too lightly the objections of some critics that an educational system will tend to reflect and perpetuate the values and goals of the dominant group within the society when he suggests, to cite one instance, that education must not only attack "every problem germane to the nature of class relations, divisions, conflicts, and objectives, but [share] throughout in their solution" (p. 81)? (3) How does the author justify the transfer from what Puerto Rican culture was and is (research-based) to what it ought to be (educational philosophy, philosophy in general, values, etc.)? In short, his larger goal of education for freedom in a free society (pp. 319-348) is not demonstrably based on Puerto Rican cultural foundations so much as on his

own credo—and this latter could be applied anywhere, with or without prior detailed knowledge of a society's culture, metaculture, or national character.

Despite the author's devotion to anthropology and cultural studies, and his claims to "participant observation," his own research activities in the present project seem overwhelmingly to have been limited to the use of loosely-structured questionnaires and the research findings of others. In the scholarly and cautious use of these data for constructing a picture of many aspects of Puerto Rican life the author has done a splendid job. And in the preparation of "proposals for educational experiments and projects" he has presented the government and the Department of Education of Puerto Rico with a series of challenging and potentially fruitful tasks and has, in this manner, demonstrated the principal value of this kind of research.

ROBERT A. MANNERS

Brandeis University

Pineapple Town: Hawaii. By EDWARD NORBECK. Berkeley: University of California Press, 1959. xii, 159 pp. \$4.00.

Hawaii's "unorthodox" pattern of non-discriminatory race relations has been extensively publicized. Less noticed has been the challenge to this pattern by the relatively segregated, race-conscious, hierarchically organized status system of Hawaii's plantations.

A former managerial employee on a pineapple plantation, now an anthropologist, has given us this brief, valuable, descriptive analysis of the social structure of the main "company town" on a 10,000-acre pineapple plantation, located on the comparatively isolated island of Molokai. Like other plantation towns in Hawaii, this community of 807 persons is highly differentiated—occupationally, socio-economically, culturally, and racially. Five-sixths of the 367 adult males are of Filipino origin, mostly hourly-paid workers born in the Philippines; a tenth are of Japanese ancestry, including a few immigrants, the majority being Hawaii-born and reared; two per cent are of Chinese, Portuguese, and Puerto Rican backgrounds. The functioning of the whole plantation depends primarily upon decisions made by or issued through eight Caucasian men born in mainland United States who comprise the managerial elite of this subsidiary of a mainland corporation.

The study covers selected technological and social changes which have taken place since the plantation was established in the early 1920s, somewhat non-comparable cultural and subgroup characteristics of the major "racial-cultural" groups, the placement of these groups

within the physical setting and social structure of the company town, and the trend from a paternalistic, ethnically hierarchical system toward a modern industrial system of ranking in which socio-economic factors increasingly dominate over "racial-cultural" group identities in a community which is becoming culturally more homogeneous.

Norbeck's connections with the plantation's top management enabled him to include pertinent facts from the company's records and insights concerning social relationships in the town, particularly as these are viewed by the Haoles (whites). The barriers to relations of intimacy and social equality between the Haoles and persons of the other groups are competently summarized. But these barriers also limit Norbeck's opportunities to gain insights about how the community's social relationships are viewed by the "non-Haoles." This is a major shortcoming of the study. It suggests that the very structure of the plantation is still such that a multi-racial research team is needed to obtain the information required for a thorough analysis of the phenomena that Norbeck has undertaken to study. Nevertheless, Norbeck is to be congratulated on this worthwhile, though partial, case study of a modern plantation town. Social scientists of other racial-group identities should undertake complementary analyses of communities of this type.

CLARENCE E. GLICK

University of Hawaii

City Life in Japan: A Study of a Tokyo Ward. By R. P. DORE. Berkeley: University of California Press, 1958. x, 472 pp. \$6.75.

Mr. Dore has given us a major work, perhaps the most important landmark in our understanding of Japan since Ruth Benedict's *Chrysanthemum and the Sword*. (The great seminal works of Sir George Sansom, two of which have appeared since the war, stand, of course, in a class by themselves.) Benedict's considerable achievement, which was an eye-opener both to ourselves and to the Japanese, represented the triumph of method and imaginative insight over brute handicap. Whether this necessity should have been turned into a virtue—and a method ("the study of culture at a distance")—is a different question.

Mr. Dore, however, is a student of culture from close up. He knows Japan and her history, he speaks and reads the language fluently, and he has spent much time there. Therefore his imaginative understanding, although in no way inferior to Benedict's, is always informed by a strong feeling for the quality and texture of life.

We should be especially happy that this is a

study of urban life. For years now Western observers have been unable to get away from the Japanese village. Partly this is because they were preoccupied with the search for the "real" Japan, which was presumed to be more manifest in the pure and undefined countryside than in the "Westernized" cities. But in part it also represents the professional reflex of the social scientist, especially the anthropologist, for whom the village is simply a more convenient and familiar unit of observation. From this perspective the city comes to appear atypical, not "truly" Japanese, corrupted, Westernized. We therefore end up with an image of Japan as a kind of homogenous folk society on some island in the Pacific. Now, all this is important and even partially true: village life is a part of national life; the folk aspects of life are part of the picture; it is even true that Japan is an island in the Pacific. But it is as misleading as if we were to describe "American culture" solely in terms of a mountain community in the Ozarks or a Mormon town in Utah, and exclude New York, Chicago, Detroit, and Los Angeles as somehow aberrant and epiphenomenal.

By leaping in where anthropologists fear to tread, Mr. Dore has given us the first serious and comprehensive study of modern urban life in Japan by a foreigner. Japanese urban sociologists have done much, of course, and Mr. Dore makes skillful use of their materials. But they have always written for an internal audience sharing their own presuppositions and knowledge. The very necessity of bringing this to life for the *moyen homme intellectuel* of the West has constrained Mr. Dore to greater insight than most Japanese urban sociologists usually show.

In method too Mr. Dore has broken with the rigid determinisms fashionable in the interpretation of Japan. Uncommitted to any particular explanatory scheme he is able to dispense with the usual "culturalogical" form of statement: "The Japanese people do such-and-such because of their culture." Culture—in the sense of ideal norms, actual norms, ideals, traditional practices—is seen as part of the total environment within which human experience takes place. But it is not equivalent to the totality of that environment. Where Benedict's lack of understanding of Japanese history—a typical defect of American social science—led her to try to define the Japanese personality, Mr. Dore, who has an English sense of history, has been able to show us the *varieties* of Japanese personality, as they are affected by historical experiences, economic pressures, institutional and legal changes, foreign ideas, the self image, and the diverse human experiences that take place within this setting. No reader will leave this

book with his original anthropological innocence, believing, for example, that the ideal norm as traditionally defined is a sufficient explanation for what actually happens.

Nor is it of minor moment that the book is written with great gusto and good sense. Although—lest some of our brethren worry about the technical side—he has made unexceptionable use of statistics, questionnaire material, tables, and tests of significance, he uses them with refreshing discrimination. His data, instead of lying lumpishly on the page in indigested tables, are interwoven throughout the text, emerging here as an insight, there as a living, throbbing human being. Which shows again, if proof be needed, that the humane, historical sense is not necessarily inconsistent with methodological rigor.

HERBERT PASSIN

University of Washington

Children of the Kibbutz. By MELFORD E. SPIRO. With the assistance of AUDREY G. SPIRO. Cambridge, Mass.: Harvard University Press, 1958. xix, 500 pp. \$10.00.

This book—the second volume in a projected series of three—deals with the second generation of an Israeli collective settlement, fictitiously called Kiryat Yedidim. The first volume, *Kibbutz: Venture in Utopia* (Harvard University Press, 1956), gives a general description of this kibbutz and is meant to provide the necessary background information for the present study. The third volume, which has still to appear, will contain an analysis of projective tests administered to a sample of the second generation.

The present study focusses on the relationship between child training in the kibbutz and the personality of the *sabra*, which is the nickname for native-born Israelis, or, in Spiro's terminology, for native-born children of Kiryat Yedidim. The kibbutz education is a rather peculiar one, because in the kibbutz, at least in the one Spiro studied, children live apart from their parents and are reared by specially trained nurses and teachers. They spend only a few hours a day with their parents and siblings, sleeping, eating, playing, and learning in nurseries and children's houses organized according to age groups. Spiro carefully describes the whole process of socialization and personality development from birth through childhood and adolescence to adulthood and tries to evaluate the function of the three main socializing agents: the nurses and teachers, the parents, and the peers. He also compares the elaborate codified body of rules on child training existing in this kibbutz with actual practice.

The fieldwork was done jointly by the author

and his wife during 1951. The study is based on systematic observation, interviews, analysis of written sources, and a number of tests. The underlying theory is largely of a Freudian nature, although few explicit statements on theory are made. As a whole, the book provides us with an excellent and detailed description of the education pattern in Kiryat Yedidim.

However, a few reservations should be made. First of all it should be pointed out that Spiro's picture is by no means representative of the kibbutz, as the title suggests it is. The author must have realized this, but his statement on this point on page xiii is not completely convincing. It should be clear that Kiryat Yedidim is, even within its own (Marxist-oriented) federation of kibbutzim, probably a rather extreme case. And this federation is only one of the three major federations of collectives in Israel. Other kibbutzim, where different child-rearing practices exist—e.g., those where the children sleep at night in the parents' rooms instead of in nurseries and children's houses—would almost certainly show different processes of personality development.

A second point of criticism concerns the fact that the author views Kiryat Yedidim as a completely isolated society, without taking into consideration the influences of the surrounding culture and its possible effects on sabra personality. To cite only one example: Spiro remarks, when speaking about feelings of inferiority among sabras, concerning their intellectual abilities, "It is hard to understand how the sabras acquired this self-image, for their intellectual interests and attainments are unquestionably higher than those of the average American high-school graduate" (p. 453), and offers a number of explanations for this phenomenon. But he fails to make the most obvious comparison, which is surely not with American high-school youth but with the actual reference group of the kibbutz sabras, namely the urban high-school youth of Israel.

Closely connected with this is a third objection, the frequency with which easy *ad hoc* interpretations are offered. One of Spiro's favorite methods of explaining elements of the kibbutz culture is to trace them back to certain patterns of the East-European Jewish *shtetl* culture, from which most of the founders, i.e. the first generation, originate. But when he turns to the second generation, he tries to explain certain traits, e.g. sabra aggression, by relating them to certain experiences in early childhood. He overlooks, however, the fact that not only sabras in other types of settlement but also first generation immigrants from Eastern Europe show this same kind of aggression, which cannot be explained by kibbutz education. Could

it be that aggression in kibbutz sabras has not only to do with certain child-rearing practices in the kibbutz but also with certain traits in East-European Jewish culture?

But despite these objections this book has undoubted merits. It is an important work, both for students of the Israeli kibbutzim and for those interested in the comparative study of culture and personality. One only wonders what reaction this book will provoke among the Israeli kibbutzim and their members, highly sensitive as they are to the views of others about their way of life.

J. E. ELLEMERS

University of Amsterdam

Parochial School: A Sociological Study. By JOSEPH H. FICHTER, S.J. Notre Dame: University of Notre Dame Press, 1958. xii, 494 pp. \$6.00.

The publication of an intensive case study of one school could be a signal event; it is ironic but true that despite our interest in the process of socialization, we have few studies of American schools available. Father Fichter's book holds out hope to those who leaf through its pages: impressive headings (e.g. Patterns of Socialization, Structures of Group Action) and detailed data-collection (interviews, observation, group interviews, tests, and questionnaires) suggest we are to read an important social document. One's disappointment on reading the book is the greater for the hope aroused.

Perhaps the major difficulty lies in the absence of a clear and consistent purpose in the research effort. In a brief introduction, the author states: "The main reason why we studied the parochial school is to present the scientific facts in an unbiased way." There is no frame presented, however, to give the thousands of facts sociological meaning; page after page of almost conversational description is presented, and partially summarized into "generalizations" at chapter endings, with minimal addition to our understanding of the nature and operation of the parochial school. Occasional reference is made to the "function" of the parochial school vis-à-vis the perpetuation of the parish, but the latter is never placed in context—it floats without anchor in the social sea.

The author suggests that the presentation of "facts" will somehow dissipate misunderstanding of the issues which surround parochial schools in America. A book such as this might serve such a purpose—not primarily a sociological one of course—by being useful social commentary. Yet in this respect the outcome is also unclear. The facts presented, one feels, are intended to refute allegations the writer thinks

unfounded, but since the issues in question are never stated, the net effect is like shadow-boxing. One wonders further if the central idea will hold water. Those objecting to religious schools will hardly change their values before the calmest description of such institutions, and those who favor them probably need no encouragement. One reading Father Fichter's book can hardly doubt that parochial school education is indeed religious education. The evidence that parochial school children favor Tab Hunter and other Hollywood luminaries in about the same proportion as those of a "control" public school hardly seems sufficient, it seems to me, to prove that the centrifugal tendencies of special schools are balanced by offsetting integrative tendencies. A book with hidden opponents can hardly be effective polemic.

This work might have value for those undertaking research on schools as a source of what Merton calls "originating questions." There are difficulties, of course, for despite the highly factual nature of the report, the data are presented in somewhat incomplete form. The full questionnaires and results are not included, though individual tables, often without the simplest cross-tabulation, are scattered throughout. Quotations from interviews are presented as "illustration" and then incorporated into the argument. These problems aside, one's curiosity is at least pricked by the heading "laymen teachers," and other anomalies present where professional control resides in officials of church rather than school. Although such questions are not treated in this rather orthodox work, the reader does, from time to time, capture the special flavor of the Catholic school.

This study underlines again the difficulty of using anthropological techniques (in community study fashion) in our own society. The techniques are all here; what is lacking is the fresh, perceptive viewpoint of a Waller or Lynd. There are no surprises in *Parochial School*.

Harvard University

DAN C. LORTIE

The Integrated Classroom. By H. HARRY GILES.
New York: Basic Books, 1959. xii, 338 pp.
\$5.00.

While the first part of this book contains information of an illustrative sort which is of sociological interest, the volume was written chiefly for grade and high school teachers and principals rather than for sociologists, to whom it will seem somewhat naive in its interpretations and pleadings. Its main purpose, obviously, is to promote tolerance and equality in the school setting. With this purpose there can be little argument from social scientists.

Of the three parts of the book, Part I summarizes findings from several studies of attitudes in various parts of the country—principally those of school board members, administrators, and teachers—and includes one such study based on a sampling from thirteen states and conducted by the author. Part II, the analysis of the data thus obtained, is elementary for the professional sociologist, but probably is necessary for teachers and administrators at the grade and high school levels. Part III is made up of discussions of methods of application of the general findings of the book in the teaching situation.

What comes most clearly from the first part of the book is that resistance to desegregation of schools is a complex rather than a simple phenomenon. Resistance did not, in every instance, indicate a lack of sympathy for the aims of desegregation but often, rather, with the methods employed.

An important contribution of the book is the chapter on "As Children See It." As the opening paragraph of the chapter states: "School integration, primarily concerns the children. Yet the overwhelming majority of the reports, letters, speeches, and even national polls taken in the first three years since the Supreme Court's decision emphasized the actions and attitudes of adults, with only passing reference to the children."

As the chapter indicates, the attitudes of children reflect largely those of parents and other elders. Yet the children's attitudes are also susceptible to change, and such change becomes the focus of concern for classroom teachers.

The author's chief research contribution was a survey of opinions of superintendents, principals, and classroom teachers in 32 towns and cities and two county school systems in 13 states regarding feelings, problems, and procedures of the school personnels towards desegregation in their school systems.

PAUL WALTER, JR.

University of New Mexico

Population and Progress in the Far East. By WARREN S. THOMPSON. Chicago: University of Chicago Press, 1959. ix, 443 pp. \$7.50.

This is Thompson's third volume on population and related matters in the Far East. In it he has rewritten and brought up to date the point of view and materials of two previous works, *Danger Spots in World Population* (1929) and *Population and Peace in the Pacific* (1940). This volume is timely (as were its predecessors) in calling attention to some of the larger implications of population growth, which

are often lost in piecemeal discussions of public affairs of this area.

After giving a very brief historical perspective of population growth and the problems of rapid economic development, the author plunges into a description of conditions in individual countries: Japan (four chapters), India (three chapters), China (three chapters), Pakistan and Ceylon (sharing one chapter), Mainland Southeast Asia and Southeast Asian Islands (one chapter each), Taiwan and Korea (one chapter). A concluding chapter is titled "Population Pressure." A wealth of factual information is presented, followed by a large and useful bibliography. The material is treated country by country, rather than on an analytical basis for the region.

What is the special place of this book in the literature? It is a *survey* and seems addressed to those with rather broad regional interests. It is an excellent introduction to contemporary affairs in this region. The reader will find that it presents him with a wide range of facts bearing on the prospects of population growth and economic development. It happens that population growth is, here, a good point of departure in examining these facts.

The breadth of treatment in this book is also one of its limitations. While it does not require the reader to be familiar with the subject, neither does it carry him very far. Its approach is not analytical; it is a description—uniformly careful, well documented, and slow of pace—of what has been done and what is likely to be done in each country. At the modest level of discussion, the book contains a great deal of wisdom; the author is eminently qualified to judge the data and their meaning. A large and varied literature is surveyed, but, for some reason, the population literature is dealt with very lightly and the uses of population data are rather perfunctory. This approach unfortunately gives the reader little guidance in performing some of the judgments for himself, should he lay his hands on some of the region's population data.

The final chapter ("Population Pressure") is the one that supplies most perspective. For most readers there should be profit in reading this one first. The term "population pressure" is something of a red herring, being used almost as a synonym for *poverty* (which, in an introductory book, seems to foster a common tendency to forget that poverty has many sources). Though this worn out term contributes nothing but confusion, there is probably no need to quibble over it. For in spite of it the author shows that what disturbs him are the political implications of continued poverty in this part of the world. On the one hand, the angry reac-

tion to poverty in crowded countries is a powerful symbol for organizing national adventures; on the other, national expansion is a nearly irresistible way of satisfying a clamor over crowded conditions. In the case of China, has this connection become a conscious part of national policy? Naturally, Thompson does not supply the answer, but the reader is left with sufficient information to wonder intelligently about this.

GEORGE W. BARCLAY

White Plains, N. Y.

Six Allies and a Neutral: A Study of the International Outlooks of Political Leaders in the United States, Britain, France, West Germany, Italy, Japan and India. By LLOYD A. FREE. Introduction by HADLEY CANTRIL. Glencoe, Ill.: Free Press, 1959. xiii, 210 pp. \$5.00.

This unpretentious survey of the main political concerns and attitudes of legislators in seven countries will be useful to anyone interested in current world affairs. It reports information of a kind which is poorly covered in the existing international communication system.

Our mass media focus on those foreign events which are important by the criterion of American interests; they do not convey a picture of the relative saliency of those events to others. Also, thanks to the emphasis on spot news reporting, the American media largely fail to put foreign attitudes into context. And finally, due to the rarity of use of polls by reporters, the media seldom give a reliable picture of the distribution of attitudes. With such deficiencies in his usual sources of information, the reader of Mr. Free's book is likely to be surprised that less than half of the Japanese Diet members would like the U.S.A. to be militarily stronger than Russia, although 71 per cent want to cooperate very closely with the U.S. and only 16 per cent with Russia. Also only 19 per cent doubted U.S. peaceful intentions as against 43 per cent doubting Russia. The common thread that makes these views hang together is a desire for enhancement of Japanese influence. Negotiation with America is not made easier for Japan if America is pre-eminently strong.

That is the sort of reportage which emerges from Mr. Free's book. He attempts to unravel the international structure and coherence in the thinking of M.P.s in India, Japan, France, Great Britain, West Germany, and the United States. The seven case studies are all interesting and would be of inestimable value to public policy making if they came to be well known. Little use is currently made in government of such opinion survey materials. U.S.I.A. does col-

lect polls, but they are undersupported and then classified—how can opinion of foreigners possibly be an American secret?—so that they do not get a fraction of the attention or expert interpretation they deserve. No one collects comparable materials from elite samples on a regular basis. By presenting an exciting example, Mr. Free's study demonstrates how valuable such elite survey material can be.

Yet social scientists will be dissatisfied with this book. It misses the opportunity to subject the interesting survey data collected to serious analysis. The only cross-break used in reporting the views of parliamentarians in a country is party, and that only sometimes. The only comparative analysis of this rare cross-cultural material (in which identical questions were used) is to place the seven countries along a quite misleading scale from neutralism to a hard alliance policy. (Indeed the great merit of the book is to show how oversimplified and ethnocentric is that very classification.) Furthermore, only occasional figures are reported, thus denying the reader the chance to do his own re-analysis of the data.

We find, for example, some figures on the frequency of mention of nuclear war and devastation in reply to a free answer question about worst fears for their country. Such answers are said to be "the top of the list" for respondents in the U.S.A. and for the following proportions of other samples polled: 40 per cent of English Labourites, 38 per cent of Social Democrats and 25 per cent of C.D.U. members in Germany, 10 per cent of the respondents in Italy, and less than 10 per cent of respondents in France and India. Such data, compiled from various chapters, tempt but frustrate the analytical reader who suspects that buried in the data are some highly significant relationships.

ITHIEL DE SOLA POOL
Massachusetts Institute of Technology

The United States and the United Nations: The Public View 1945-1955. By WILLIAM A. SCOTT and STEPHEN B. WITHEY. National Studies on International Organization. Prepared for the Carnegie Endowment for International Peace under the auspices of the Survey Research Center, University of Michigan. New York: Manhattan Publishing Co., 1958. 314 pp. \$3.00.

This book represents an ambitious attempt to examine the data on U. S. public opinion regarding the United Nations over a period of a decade and to wring meaning from these data which, unfortunately, the poll-takers never quite built into them. Sociologists can nevertheless learn a great deal from the attempt.

Data from the various national polling agencies were brought together to reveal trends in (1) support for the U.N., (2) extent of information seeping through to the public, (3) levels of satisfaction with the U.N., (4) expectations concerning the U.N., (5) understanding of U.N. structure and powers, (6) awareness of the world affairs context of U.N. action. In addition to a chapter on each of the above topics, there are interesting chapters on "Social Characteristics and Reactions to the U.N.," "Interrelations Among Reactions to the U.N.," "The Social Psychology of Public Reactions to Foreign Affairs: A Theoretical Interpretation." The introductory chapter, entitled "Public Opinion and U. S. Foreign Policy," is by the president of the Carnegie Endowment for International Peace, which sponsored the study.

Although the authors are both psychologists and the orientation of the book is psychological, it will be of interest to many sociologists. When even among those who thought in 1953 that the U. S. should stay out of world affairs, 70 per cent favored continued U.N. membership, and when in 1954, 51 per cent said the U. S. should stay in the U.N. even if "after several years it turns out that the U.N. isn't able to do the things it is set up to do," the sociologist familiar with alternative theories of organization may conclude that support for the U.N. is based on other than instrumental considerations.

Two features of the public opinion trends reported in this book impressed this reviewer as basic to an understanding of the total picture. (1) Despite fluctuations, there has been an upward secular trend in the percentage of Americans who say they are satisfied with the work or progress of the U.N. (2) The principal function of the U.N. has been redefined. It was originally seen as an instrument for preventing war, but it has come to be evaluated as an instrument for stopping Russia (or perhaps merely for expressing U. S. antipathy toward Russia). Between March 1945 and September 1946—only one and one-half years—there was a decline from 82 per cent to only 20 per cent who felt that the U.N. could prevent war for 25 years. In August 1953, the most frequent reason given for wishing to strengthen the U.N. was "A powerful United Nations is the only way to handle Russia," and in March 1954, of those able to mention any U.N. function, about 90 per cent in each educational category mentioned a military or power function.

The book may be of especial interest to students of the sociology of knowledge. If the definition of the situation with which most Americans view the world has undergone such marked change since World War II, it is interesting to discover the kinds of questions pollsters

have thought it pertinent to ask and the kinds they have not asked. Was it cultural lag, perhaps, that prompted N.O.R.C. and S.R.C. to ask people nine times between 1946 and 1951 whether "this country has gone too far in concerning itself with problems in other parts of the world?" What underlying assumptions have led pollsters to believe it was meaningful to ask, "Do you think it will be best for the future of this country if we take an active part in world affairs, or if we stay out of world affairs?" Or, conversely, why have certain data which would have been very meaningful *not* been collected? The authors note that most of the data concerning international control of atomic energy were collected during 1946-1948. "No comparable material is available concerning the period since the advent of the H-bomb and since the Soviet Union has developed its thermonuclear weapons." Whereas questions regarding the probability of achieving the more pretentious U.N. goals have been asked repeatedly, "most of the questions relating to modest goals were asked in the early stages of U.N. history." And, "The only available data relating education to support for specific U.N. actions are on the subject of fighting in Korea."

After wading through dozens of tables of percentaged data, one might well ask further, what social (or economic) factors have caused so little use to be made of sophisticated psychometric techniques in public opinion polling?

WILLIAM R. CATTON, JR.

University of Washington

Our Public Life. By PAUL WEISS. Bloomington: Indiana University Press, 1959. 256 pp. \$4.50.

Philosophy has ever guided the study of man, and it is a matter for regret that at the present time few, if any, from the ranks of either philosophy or sociology seek to do what was done so valuably a generation ago by such men as Hobhouse and Wallas in England, and by Dewey, Mead, and Cooley in this country. These men dealt unabashedly in speculative fashion with the larger problems of society and the methodological issues involved. No one acquainted with the history of American sociological thought is likely to underestimate the influence upon its direction and development of such books as *Human Nature and Politics*, *The Public and its Problems*, and *Mind, Self and Society*.

Today, apparently, such enterprises are unfashionable. In sociology a compulsive and pandemic empiricism has either choked off minds of philosophical bent or else driven them into the dubious construction of "systems" or "theories" which usually end up bearing no more

relation to science than to philosophy. The situation is scarcely better in the field of philosophy. There a desolating positivism has shattered the historic problems of society, leaving only shards of "meaning" and "non-meaning."

So it is a pleasure to see an American philosopher take up in manly, head-on fashion some of the problems which for twenty-five hundred years have been a stuff of social philosophy. The object of the book is to present a systematic account of the nature of such structures as society, culture, and civilization. In quick succession (the book is based upon lectures given at the University of Indiana), the author treats of social classes, rights, law and sovereignty, natural law, and the state. He does this through what he calls the method of dialectical construction. If the result has any merit, Professor Weiss writes, "it should provide a kind of ideal model which will enable us to see more clearly than otherwise just what public structures, values, and connections there might conceivably be, and just what it is that one might find valuable to institute in the field of public affairs."

No one, surely, will take issue with either the relevance or the potential fruitfulness of the method. From Plato to Royce, this method has been the means of uncovering attributes of the actual that a merely descriptive approach would trample over like so many cattle over a flower bed. Given a mind of both power and learning, the dialectical method can yield much to our understanding of those larger problems of society which give meaning and direction to the small ones.

Of the power of mind behind this book there can be no doubt. The criticisms, all too brief, of the empirical and analytical methods can be read with profit by any social scientist. The treatment of rights is subtle, penetrating, and lucid. Bradley himself might blink respectfully at the level of discussion of alienation of rights. Also impressive, and frequently original, is the chapter on units of civilization.

The least satisfactory chapters in the book deal with sovereignty and the state. Quite apart from the fact that the level of analysis falls below most of the rest of the volume, there is little to suggest that the writings of such men as Tocqueville, Weber, Figgis, or Maitland have left any mark. And how an American philosopher can write a book on public life without even a reference to Dewey's wonderful little volume on the subject is hard to explain even when one has allowed for the broad differences in world view.

Such omissions reflect the book's chief defect: lack of evidence of serious concern for what others, especially modern political scientists and sociologists, have thought on matters of common ground. We rightly look to philosophy to remind

us of first principles and the true dimensions of problems, and this Professor Weiss does efficiently, if hyper-didactically, but there is surely an obligation on the philosopher, in turn, to be reasonably informed of the works of other toilers in the vineyard who also trace their descent from the Greeks.

ROBERT A. NISBET

University of California, Riverside

Anthropology and Ethics. By MAY EDEL and ABRAHAM EDEL. Springfield, Ill.: Charles C Thomas, 1959. 250 pp. \$5.50.

An anthropologist and a philosopher have joined forces in the writing of this book on ethical values, a territory where the two disciplines overlap. The authors are well informed and competent, not only in their special fields but in history and psychology, and as a result a rich book has emerged.

The most interesting feature of the book for the social scientist is the ideas about the kinds of information needed for an adequate description of a value system. Anthropologists will find this part of the study a mine of suggestions for further research. The Edels define "moral" phenomena broadly so as to include prescriptions for behavior, the praising and blaming of character-traits and attitudes, and goals and experiences regarded as desirable. They describe various ways in which value-systems can differ, and it is from this that their proposals for an adequate description emerge. Among the dimensions of value-systems to which they call attention are the following: the main ethical concepts (e.g., "sin") and the intellectual areas with which they make contact, the content of the prescriptions and how strongly they are enjoined, the generality of the prescriptions, the extent to which models are employed, the area of life which is morally emphasized, the type of sanctions used, the importance given to personal moral decisions, the emotions typically involved, the persons regarded as belonging to the moral community and held responsible for their actions, and the kinds of justification typically offered for moral judgments.

The authors are interested in recurrences of configurations, in causally connected organizations, and are concerned with what is behind these patterns. They offer no general explanatory suggestions, but emphasize a variety of factors which evidence suggests are somehow involved.

Another center of attention is the extent and significance of diversity among moral systems. The authors summarize the most careful types of contemporary anthropological thought on this matter. They survey the salient facts about attitudes toward incest, aggression, distributions

of goods, and obligations to children; and they both explode some widespread conceptions about the scope of disagreement and formulate some detailed problems which call for further investigation.

A major thesis of the authors is that awareness of material of the above sort must partially dissipate the supposition that there is an unbridgeable chasm between science and facts on the one hand, and values on the other. For science, they point out, can overturn the factual assumptions on which moral systems are sometimes based. Further, science can show that moral systems can either frustrate or fulfill the biological and social needs of the men who live by them. Science can also uncover possibilities hitherto not envisaged. Of course, the authors cannot claim that such scientific showings establish any value propositions without the assumption of some value judgments, e.g., the judgment that needs should be satisfied. The authors would not contest this, but would point out that some basic value judgments must be accepted because they spring from perennial and essential human needs. Given this fact and the scientific showings, they think, a large measure of agreement in ethics can be achieved, although they happily admit that there may be alternative but equally defensible systems of moral values. In the opinion of the reviewer, this theory is only part of the story, and needs supplement by further analysis of the function of ethical discourse and the logic of ethical reasoning. Nevertheless, the theory (which Professor A. Edel discussed at greater length in his earlier book, *Ethical Judgment*), is a sophisticated one; and the subtlety and scope of the social science discussions of the present book are matched by the competence of the philosophical part of the volume.

RICHARD B. BRANDT

Swarthmore College

The World of Man. By JOHN J. HONIGMANN. New York: Harper & Bros., 1959. xii, 971 pp. \$7.50.

For seven and one-half dollars, the 880 pages of text, 60 pages of bibliography, and 30 pages of indices in *The World of Man* are quite a value. The book will probably be found more useful in courses in cultural rather than general anthropology, since the less than 200 pages on physical anthropology and prehistory (placed somewhat surprisingly at the end rather than the beginning of the work) are sketchy and poorly organized. They hardly measure up to the very high standards set in the parts of the book devoted to cultural or social anthropology.

Honigmann's presentation in the parts on cultural anthropology is indeed a distinguished

contribution, comparable in scope and sophistication to Kroeber's classic *Anthropology*. Lucid, thoroughgoing, and systematic discussions are given of anthropological method, the nature and scope of anthropology, and the nature, processes, and divisions of culture. The six chapters on methodology are especially welcome, since so many introductory texts give the subject only scant attention. The exposition on methodology seems to the reviewer to be marred only by Honigmann's insistence on the familiar history-science dichotomy, which many anthropologists would regard as no longer very fruitful.

Throughout the book, Honigmann forbears from making himself the advocate of any particular "school" of thought; nor is he concerned—as some recent textbook writers have been—with identifying and labelling the various schools. He performs, instead, the more useful service of drawing upon and integrating many varieties of anthropological thought. And he also ably draws upon the contributions of behavioral sciences other than anthropology.

It should perhaps be noted that Honigmann is not always entirely successful in synthesizing different points of view. For example, he endorses the view that "all cultures have an equally long history and none can be taken as closer than any other to man's earliest ways of life" (p. 72). At the same time, he recognizes that geographical marginality or isolation favors the persistence or "arriance" of culture (pp. 190, 750, 788, and *passim*). Surely this implies that some cultures are indeed more similar than others to earlier ways of life.

However, there seem to be but few such inconsistencies in the book, and the ones that occur may be useful to the teacher of cultural anthropology for stimulating the thinking of students. Although the teacher will no doubt want to disregard, elaborate, or reorganize some parts of *The World of Man*, he will find in it valuable passages on almost any particular topic that he might want to discuss and might want students to read about.

Among the minor criticisms that may be noted is the fact that the book contains considerable unnecessary citing of authorities in support of either vague generalities or very commonplace statements. Thus, Sol Tax is cited to the effect that anthropology "is bounded only by the limits of what anthropologists do and use," Stanislaw Klimek is given as the authority for the statement that a culture area "represents a portion of the earth's surface in which a series of communities share a significantly large number of traits," and a reference to an article by Alexander Gerschenkron follows the statement that "perhaps people are not born for the machine." It may be noted also that some of the illustrations

in the volume are accompanied by unduly cryptic captions. For example, the picture on page 12 of two Maori school children sitting by a Maori wood-carving is accompanied by only the following words: "Practically Everything Man Does Is Conditioned by Social Factors (courtesy, New Zealand Embassy)."

ANDREW P. VAYDA

University of British Columbia

American Marriage and Divorce. By PAUL H. JACOBSON. In collaboration with PAULINE F. JACOBSON. New York: Rinehart & Co., 1959. xviii, 188 pp. \$12.00.

Under the leadership of Louis I. Dublin, the Metropolitan Life Insurance Company for many years has published, in its *Statistical Bulletin*, popularly phrased articles on American marriage and divorce patterns and demographic aspects of the family. In the book under review, Dr. Jacobson, who is now head of the company's population and family research section, utilizes a very considerable amount of widely scattered and irregularly appearing information collected from state, city, and Federal publications and from other, sometimes unpublished, sources.

The first half of the book treats of marriage statistics and is a revision of the author's doctoral dissertation at Columbia University; some of the other sections have appeared in whole or in part as articles in the *Statistical Bulletin* and elsewhere. About one-third of the book is composed of tables. The monograph has much useful information for the specialist in this field of statistics, but it hardly contains the "great wealth" of material claimed for it. Some topics are barely touched upon; others on which data are available are not treated at all; and some others of marginal interest are expounded at length.

The discerning reader will probably be perplexed and alerted by the valid and highly critical viewpoints expressed in the book about existing data in this field (described as "woefully inadequate")—after which the author proceeds to give a statistical accounting for the whole of the United States, and pretentiously so labels nearly all of his tables.

In spite of much real merit in the materials as a whole, and in spite of the principal author's intimate knowledge of the field and his technical capacity, this work does not measure up to some scientific and scholarly standards.

What are the principal defects of this work? First, the sources of the data used are not spelled out to enable independent verification and evaluation. (Sources are given in part, however, in the author's unpublished dissertation). Most of the tables carry an omniscient foot-

note, "Source: Estimated by the author," or a variant of the same. *Second*, and this is a view Dr. Jacobson has expressed elsewhere, there is no way of guaranteeing that the *particulars* of the data, comprised of a variable amalgam of states and cities and fluctuating widely in coverage from year to year (a low of 10 per cent coverage is not unusual), are at all "nationwide" or "representative" of the country as a whole, as the author claims. And, oddly enough, while being critical of the material, the author takes important recourse to population census data by marital status instead of sticking to his marriage and divorce facts. *Third*, there is not only no bibliography, but also there is no citation of pertinent references in the literature on particular topics. In some cases the omissions almost appear intentional, as with regard to nuptiality and marriage age. Hence, too, there is no real integration of previous findings. *Fourth*, and this is perhaps the most serious defect, the author, in his attempt "to bring out the underlying patterns and their sociological significance" (book jacket), takes no recognition of certain methodological principles explicitly stated in the literature on marriage and divorce. To mention only one principle, the importance of studying "first" (primary) marriages is something the author never squarely faces, perhaps because of the great lack of material so refined. And yet, as has been said so often, the circumstances surrounding first marriages (their variation and outcome) are undoubtedly the chief focus of interest and significance in any marriage and divorce study.

Illustrating the failure to follow sound principles and to assess the literature is the headlined reappearance of the author's article on the relationship between childlessness and divorce. The existence of a rather contradictory study is not even acknowledged.

Perhaps the author attempted an almost impossible task in view of the kinds of statistics available. But it would have been better had he explicitly and disinterestedly assayed the material, taking the reader into his confidence, and not overstating his findings.

THOMAS P. MONAHAN

The Municipal Court of Philadelphia

The Making of Society: An Outline of Sociology.

Edited by ROBERT BIERSTEDT. Revised Edition. New York: Random House (Modern Library), 1959. xx, 557 pp. \$1.65.

Of the making of books of selections or readings drawn from representative works in so-

ciology and related fields there has been, and there is likely to be in the foreseeable future, no end. That is precisely what the present volume is (in spite of the slightly misleading secondary title): a book of readings. Like all such books, it will not be entirely satisfactory to any sociologist (not even its editor); each will wish that a few of his favorite selections had been included, even if this necessitated the omission of some of the admirable selections that have been chosen. The present reviewer feels that he could hardly do better in describing the book than to quote, with slight abridgment, the two opening paragraphs of the editor's preface.

This is a new edition of a book edited by V. F. Calverton and published in the Modern Library in 1937. Of the sixty-one selections that Calverton included in the first edition, however, I have retained only eighteen . . . and I have added twenty-seven that did not appear there, making a total of forty-five in the present volume.

Calverton's predilections in the mid-thirties were Marxian. . . . My own predilections in the late fifties belong to a tradition that is both Comtean and orthodox, one that considers sociology "value-free" in general and politically neutral, and one finally that emphasizes, rather than erases, the distinction between sociology on the one hand and social and political philosophy on the other.

Selections from "social and political philosophers" are, nevertheless, not entirely omitted from this volume; especially is this true of Part I, "Classical and Medieval Statements," where Plato, Aristotle, Cicero, St. Augustine, St. Thomas Aquinas, Dante, and Marsilius of Padua are represented. The chronological sequence continues down through George A. Lundberg, Howard Becker, and Talcott Parsons.

If the reading of these selections, which seem to this reviewer to be on the whole admirable, should stimulate students to go on to examine at greater length the works of at least some of the authors represented, the book would have justified itself. Accepted as a substitute for more extended reading, this book would render a disservice to its readers. Bierstedt's ten-page "Introduction" is excellent and his inclusion of brief biographical information about the author at the beginning of each selection should be helpful. The format, familiar to all those who have seen recent issues of the many titles in the Modern Library, is attractive and convenient, and the print is reasonably legible. All in all, Professor Bierstedt and the publishers are to be congratulated on this volume.

FLOYD N. HOUSE

University of Virginia

BOOK NOTES

Special District Governments in the United States. By JOHN C. BOLLENS. Foreword by JOHN M. GAUS. Berkeley and Los Angeles: University of California Press, 1957. xiv, 280 pp. \$4.50.

In the United States there are over 79,000 special district governments, which constitute about two-thirds of the total number of governmental units of all kinds. In the fiscal year ending in 1955 they spent approximately \$9.8 billion, outranking the collective expenses of counties, townships, and towns, and approaching the \$10.5 billion figure for cities. And in that year 1,570,000 people were professionally engaged in district activities. Despite their importance, we know relatively little about special district governments. Thus, this book by Professor Bollens is most welcome.

After an introductory chapter describing the general characteristics of special districts, Professor Bollens discusses metropolitan, urban fringe, coterminous, rural, school, and dependent districts and authorities. A final chapter on the status and prospects of special district governments and a commentary on bibliography complete the volume.

Each chapter contains a description of the structure and function of the special district government under consideration, a brief analysis of its development, its sources of funds, its methods of control, and its administration, together with several examples and a critical evaluation. The extraordinary character of some of these governments provides research possibilities for sociologists. This is an important and thought-provoking book.—WENDELL BELL

A Study of Cultural Change: Rural-Urban Conflicts in Norway. By PETER A. MUNCH. Oslo: H. Aschehoug and Co. (W. Nygaard), 1956. 104 pp. No price indicated, paper.

Norway has a vigorous national peasant movement with its own political party, youth organizations, schools, newspapers, and even its own language. In a rather long-winded introduction, Munch ranges widely, expressing his recipe for adequate sociology and detailing three alternative conditions whereby a group distinguishes itself symbolically from a wider population. Next comes a well-written but only sporadically documented essay on how the Norwegian Peasantry have come to think of themselves *vis-à-vis* the rest of the nation. Finally there is an account of local conditions in one mountain valley, mainly dealing with the decline of an old administrative center in the face of com-

mercial and later social competition from a railway terminus.

The sociology of modern Norway is still largely *terra incognita* and any gleam of light is welcome. But the last two parts of this book deal with different social processes and are not sufficiently related to one another, while the first part is far too discursive a theoretical exercise for the modest use made of it.

The value of the book lies in the middle essay. It might have helped to consider other situations outside Norway where a language has become a symbol of group identity, as with Afrikaners in South Africa or even the Scots in England.—J. A. BARNES

The Older Worker in Industry: A Study of the Attitudes of Industrial Workers Toward Aging and Retirement. By G. HAMILTON CROOK and MARTIN HEINSTEIN. Berkeley: Institute of Industrial Relations, University of California, 1958. vi, 143 pp. No price indicated, paper.

This monograph reports the results of a survey of 846 blue collar industrial workers conducted in the San Francisco and Los Angeles areas in 1952-54. Open-ended questions were asked to elicit attitudes toward jobs and working, age and aging, future retirement expectations, and different retirement policies. Results are straightforwardly presented in tabular form, with discussion. Cross-tabulations are made by "objective" data on age, sex, occupation, education, marital status, and work performance ratings by supervisors; existing retirement policies are appraised in light of the results. The entire study appears methodologically sound, suffering from no more than the usual unavoidable difficulties of any attitude survey. Questions of the "what might you do if" variety are kept to a minimum. Procedures are discussed in commendable detail in an appendix which also includes a full reproduction of the interview schedule.

Highlights of the findings include considerable evidence that proximity to retirement age is associated with negative attitudes toward retirement, and some indications that self-perception of age is an important determinant of work performance level. Problems of interrelating chronological, physiological, and psychological age are viewed as raising questions about rigid age specifications in both hiring as well as retirement practices. On the whole, this study is sociographic rather than analytic. As such, the data which it provides should be of considerable value to anyone interested in occupational gerontology.—STANLEY H. UDY, JR.

Étude Analytique de Stratification Sociale: Les Agents des Contributions Directes en Belgique.

By the CENTRE INTERUNIVERSITAIRE DE SOCIOLOGIE. Liege: Vaillant-Carmanne, 1959. 116 pp., 149 tables. No price indicated, paper.

A Belgian inter-university research center reports in this monograph the results of a questionnaire distributed to over 6,000 public administrators in the tax branch of the Ministry of Finance.

One emphasis of the study is on the internal structure of the organization, particularly the professional training, self-image, and morale. The other concern is with social stratification, the assaying of the social class origin of these administrators.

Considerable change seems to have taken place in this Ministry over the past hundred years. There has been an increasing professionalization which is reflected in the high morale and dedication to the organization. The emphasis on professional competence acquired on the job rather than on formal education, has resulted in a broadening of the social class base with most of the men now coming from "modest" backgrounds, a condition apparently not true of the other Ministries.

The brief questionnaire and almost one hundred pages of tables of cross-tabulations are included in the appendix.—ARNOLD LEVINE

The Comparative Study of Religions. By JOACHIM WACH. Edited with an Introduction by JOSEPH M. KITAGAWA. New York: Columbia University Press, 1958. xlviii, 231 pp. \$4.50.

This book contains lectures, earlier versions of which the late Professor Wach delivered in India in 1952; they were not ready for publication when he died in 1955. Professor Kitagawa, a colleague, undertook the final version, but it is still too fragmentary and compressed in style: the depth and complexity of Wach's thought are not entirely conveyed by the work. The titles of the chapters give some indication of the contents and theme: "Development, Meaning and Method in the Comparative Study of Religions," "The Nature of Religious Experience," "The Expression of Religious Experience in Thought," "The Expression of Religious Experience in Action," "The Expression of Religious Experience in Fellowship." Wach was a theologian and a philosopher and this was the beginning point of his sociological work on religion. His last work, despite its many limitations of form, serves us well if it reminds us of the evaluative presuppositions of sociologies of religion, and of the breadth of historical and comparative knowledge required for work in this field. Professor Kitagawa has supplied an instructive and thoughtful

sketch of Professor Wach's life and thought.—NORMAN BIRNBAUM

Social Control and the Foundations of Sociology.

Edited by EDGAR F. BORGATTA and HENRY J. MEYER. Pioneer Contributions of Edward Alsworth Ross to the Study of Society. Boston: Beacon Press, 1959. xii, 211 pp. \$4.50.

This re-publication of selected portions of virtually all chapters of E. A. Ross' *Social Control* and of five of the eleven chapters of *Foundations of Sociology* signifies an increasing appreciation of the history of the discipline. The editors have contributed a useful, four-page biographical sketch, a short three-page interpretive preface indicating Ross' place in American sociology, and brief introductions to each of the three sections of *Social Control* and to *Foundations* generally. Their introductions are devoted primarily to a defense of their omissions of illustrations and materials they consider typical of the intellectual climate around 1900. Perhaps least justifiable is their exclusion of the chapter from the *Foundations* on "The Social Forces," which they acknowledge as having only "some interest historically" and which is, indeed, a crucial link between Ross and the theorization of his era. Paradoxically, they laud Ross for his "presentation of ideas and materials in historical context," yet fail to avail themselves of a unique opportunity for doing just this for Ross. An introductory elucidation of Ross' intellectual presuppositions in relation to his time—beyond the fragmentary remark that he "falls in line with the interactionist approach of Baldwin, Cooley and Mead to the socialization process"—would have signally aided "the present generation of students" in recognizing and appreciating "his clear and incisive analyses of basic theoretical problems."—GISELA J. HINKLE

Spotlight on the College Student. Edited by MARGARET L. HABEIN. A Discussion by the Problems and Policies Committee of the American Council on Education. Led by DAVID RIESMAN, PHILIP E. JACOB, and NEVITT SANFORD. Washington, D. C.: American Council on Education, 1959. 89 pp. \$1.00, paper.

This pamphlet is an edited transcript of a meeting sponsored by the American Council on Education in June, 1957. Professors Riesman, Jacob, and Sanford presented papers, each of which was followed by a general discussion on the part of the assembled college presidents, other educators, and students.

The three papers reflect the characteristic interests and viewpoints of their authors; the ensuing discussions exhibit that eagerness for sharing and for coming to grips with something

which mark the professional round-table. Quite intentionally, this stimulating concourse serves to re-emphasize how partial (in both senses) is our knowledge of that species whom we suppose to instruct.—M. S. O.

Memo to a College Trustee. A Report on Financial and Structural Problems of the Liberal College. By BEARDSLEY RUMMLER and DONALD H. MORRISON. Prepared for and Transmitted by The Fund for the Advancement of Education. New York: McGraw-Hill Book Co., Inc., 1959. vii, 94 pp. \$1.00, paper.

The authors of this "memo" survey the possibilities of, and obstacles to, the rationalization of the higher education industry. They argue that to meet the inevitable increased student-faculty ratios the liberal arts college must plan boldly to double faculty productivity (and, thereby, salaries) through schedule and curricular rearrangement. While recognizing the legitimacy of the small course and seminar, they would offset these inefficient operations by more large courses. (Because the instructor lacks time and the college lacks graduate assistants, student work in these courses would presumably be limited to taking "objective" tests.) Though they see few arguments against bigger classes, they do perceive plenty of obstacles thereto in conservative, anarchic, and faction-ridden faculties. Hence they propose to vest authority for curricular planning in some variety of administrative-legislative "mechanism" or super-committee.

The discussion is generally stimulating, often shrewd, and not insensitive to faculty problems and mood. Though addressed to a hypothetical trustee, this booklet should prove of interest to anyone connected with undergraduate education.—M. S. O.

Wissenschaftliche Grundlagen Soziologischer Denkens. By E. K. FRANCIS. Bern: A. Francke ag Verlag; München: Leo Lehmann GmbH Verlag, 1957. 143 pp. No price indicated, paper.

The purpose of the author's lectures was to introduce German students to the basic concepts of empirical sociology prevalent in America. The lectures cover history, sociological method and theory, the relation of general sociology to psychology and anthropology, "micro-macrosociology," social stratification and social change.

Of interest to the reviewer is the author's implied observation that European positivism and the *Verstehenssoziologie* of Tönnies and Max Weber—which found their most intensive development in America—are now returning,

"Americanized," to Europe and particularly to Germany as empirical sociology.

One must admire Professor Francis' ability to present in a small volume the "consensus scholarum" that basically exists in spite of the many conceptual divergences within the field of general sociology. This ability is the more admirable if one appreciates the difficulty in finding German equivalents for American sociological concepts. The author emphasizes throughout that the goal of any empirical science is to establish laws based upon the possibility of theoretical explanation. He argues that within empirical sociology, principles rather than laws are dominant, and that a synthesis of the fragmentary empirical sociological knowledge is still a task of the future.—RICHARD O. NAHRENDORF

Readings in Medical Care. By the COMMITTEE ON MEDICAL CARE TEACHING OF THE ASSOCIATION OF TEACHERS OF PREVENTIVE MEDICINE. Chapel Hill: University of North Carolina Press, 1958. xxi, 708 pp. \$6.50.

With a few notable exceptions, sociologists have only recently begun to report studies dealing with the organizational and administrative aspects of medical care in the United States. Over the past several decades, however, members of the health professions have produced an extensive body of descriptive, evaluative, and programmatic literature on the subject. The selections contained in *Readings in Medical Care* were drawn primarily from such literature, in order to provide a ready sourcebook for students, teachers, practitioners, and administrators in the health professions. Although many of the selections therefore deal with topics and problems—for example, care of long-term illness, medical care insurance—that are somewhat tangential to the main concerns of most sociologists, no sociologist who professes serious interest in the organization of medical care can afford to be unfamiliar with the contents of the volume as a whole.—M. E. W. G.

Hough, Cleveland, Ohio: A Study of Social Life and Change. By MARVIN B. SUSSMAN and R. CLYDE WHITE. With the collaboration of ELEANOR K. CAPLAN. Cleveland, Ohio: Western Reserve University Press, 1959. x, 95 pp. \$3.50.

This monograph presents the results of a social survey of an area of ten census tracts in Cleveland, Ohio. It provides a wealth of factual data on such items as population characteristics, "social ecology," houses and households, occupations and employment, attitudes toward community services, participation in community

life, "help in time of trouble," and crime and delinquency; it also shows some changes that have occurred between 1950 and 1957. The basic data were derived from 401 interviews with a random sample of heads of households drawn from the City Directory. The 401 cases represent 1.92 per cent of the households of the area.

It would appear that in many instances the investigators attempted too much with too little; it is often doubtful whether the failure to demonstrate a significant relationship is due to the absence of the relationship or to the inadequacy of the sample. Fortunately, many important conclusions reported do not suffer from this deficiency. There are many data of interest to the urban sociologist, but theoretical implications are very faintly indicated, if at all, and must be dredged out of a deep pool of bare facts by the reader himself. It is now forty years since Roderick D. McKenzie made a study which was subsequently published as *The Neighborhood: A Study of Local Life in the City of Columbus, Ohio*. In the intervening four decades, sociologists, if one may judge by these two books, have become more sophisticated statisticians, but have regressed in theoretical interpretation of results.

It is, however, probably unfair to compare this work with that of McKenzie regarding sociological theory because, as the authors state, the primary purpose of the study was to provide factual data for "planning, directing, evaluating or supporting new and old social and welfare service progress in Hough." If the results presented are interpreted with discrimination, this carefully done survey should achieve this purpose.—CHRISTEN T. JONASSEN

The Origin of Civilized Societies. By RUSHTON COULBORN. Princeton, N.J.: Princeton University Press, 1959. x, 200 pp. \$4.00.

This is a contribution to one of the oldest guessing games in human history, namely, "When and where did civilized society arise?" The origin in question is essentially an evolutionary development, extending over long periods of time and marked by an infinitude of cultural variations. Even those who lived through these changes would not have known when they ceased to be "primitive" and became "civilized." Today, thousands of years later, the record is too dim for more than informed guesses. Professor Coulborn fully understands this. Though his references show him to be an expert in this field, his work abounds in such words and phrases as "probably," "may have been," and "not very likely." There are also numerous suppositions, tentative postulates, and confessions of ignor-

ance. His data reveal many gaps of centuries or even millenia; and at one point he finds that a civilization arose during "a gap in the record." The speculations advanced thus have little interest except for those engaged in the same guessing game.

The author struggles with the problem of how to distinguish primitive from civilized society but seems to find no criterion that will reveal the time and place of origin of the latter. He accepts more rapid rate of cultural change and cyclical ups-and-downs as distinguishing the civilized society, but fails to note that neither of these throws any light on the question of origin.

The novel features of this work, if any, are the outright rejection of what the author thinks of as "materialistic" explanations and the effort to establish a "new religion" as a—or even *the*—basic factor in the rise of a civilization. His evidence is wholly suppositious and not well grounded in theoretical postulates. How and why the new religion came into operation and how it was made acceptable over a wide area remain mysteries. In his effort to escape all brands of "materialistic" thinking the author robs himself of any tenable approach to social dynamics.—FRANK H. HANKINS

Heredity and Evolution in Human Populations. By L. C. DUNN. Cambridge, Mass.: Harvard University Press, 1959. viii, 157 pp. \$3.50.

First of the "Harvard Books in Biology," this is a brief and semi-popular introduction to the biological processes of race formation. It sets forth the Mendelian principles and then gives illustrations, drawn in part from the author's own researches, of how through mutation, gene flow, genetic drift, and physiographic and social isolation and selection, human varieties came into existence. Much space is given to blood groups and the specific adaptations of sickle-cell and Mediterranean anemia genes. To view a "race" as a group transmitting a larger or smaller "pool of genes" does not invalidate the differentiations made by the physical anthropologist, but it shifts the criteria of classification from physical trait or phenotype to the trait's genetic basis. In a final chapter the difficulties and prospects of eugenic controls are assayed, with emphasis on the break-up of isolates.

The book gives no hint of the vast array of researches on human inheritance. It even leaves the reader in the dark as to some of its own sources. It is usually clear and didactic in style, though the statement on cousin marriages (pp. 88-9) fails adequately to distinguish phenotype and genotype. Sociologists should find it of considerable interest.—FRANK H. HANKINS

O Processo de Urbanização No Rio Grande Do Sul. By LAUDELINO T. MEDEIROS. Porto Alegre, Brazil: Universidade Do Rio Grande Do Sul, 1959. 64 pp. No price indicated, paper.

The author's stated intent is to provide a panoramic introduction to the study of urbanization in Rio Grande Do Sul, Brazil. This modest effort towards the formulation of some general hypotheses concerning the social process of urbanization utilizes as a guide line the classic approach to urban sociology of the Chicago school. Curiously, Professor Medeiros cites Louis Wirth as quoted by Donald Pierson, but refers neither to Park nor to Ogburn.

In the light of increasing interest in the so-called underdeveloped countries, the topic is indeed timely. Among the propositions presented, the following are the most interesting: (1) Regional zones of resistance to demographic expansion, rooted in the socio-economic patterns of life, persist in the face of cultural change conducive to the restructuring of the regional economies. The decade 1950-1960 will put this hypothesis significantly to test. (2) Continuation or intensification of the present trend in migration from the state warrants the prediction that the region will acquire the characteristics of stationary populations, and that the draining off of the rural elite will be reflected in inadequate elite mobility. (3) City-regional ties have given rise to a profound two-way influence; there is no indication of any diminution in this influence, nor has it been more than partially modified by the development of the techniques of (mass) communication.

A series of maps and an interesting table merit examination. The volume suffers, inevitably, from the defects inherent in combining introductory and advanced materials. The author's definition of "culture" is unclear; there is a weakness in his occasional apparent resort to single factor analysis where multiple factors operate. A capsulized cultural history of the state commends itself. In all probability Professor Medeiros' future reports of the research in progress will clarify and sharpen the focus.—JANICE W. HARRIS

Les paysans et la modernisation de l'agriculture: Compte-rendu d'une enquête pilote. Par HENRI MENDRAS. Paris, France: Centre National de la Recherche Scientifique, 1958. 148 pp. No price indicated.

The region chosen for this careful pilot study of French peasant attitudes toward the introduction of modern farming techniques is the Sundgau, and area of the Upper Rhine situated in the extreme south of Alsace, bordering Switzerland. A sample of 300 peasants, owning farms

of at least six hectares, were interviewed. Seventy per cent of the peasants were over fifty years of age; 72 per cent farmed less than fifteen hectares. In only 44 per cent of the cases was the entire family income derived from agriculture; small trading, artisan occupations or daily employment in the textile, chemical, or automobile industries of the neighboring cities supplemented the prevailing agricultural economy. Although two-thirds of the respondents went to the city rarely and the communities were in general outside the cultural stream of the country, they kept informed of the world outside through the press and the radio.

On the whole modernization has been slow. Some mechanization has been introduced, the tractor having prestige value, but draught animals persist. Improved methods of stock-raising or farming have been slight. Ignorance, the small size of the holdings, and pessimism as to the future of agriculture account in part for the reluctance. If modern techniques and equipment are to be introduced, the Sundgau peasant is inclined to think that they should come through state intervention and aid, not through his own initiative. Although the study points to the primordial role of the economic and social structure in conditioning the attitudes of these Alsatian peasants, certain psychosocial factors, such as resistance to out-group ideas of progress and a desire to continue in the traditional way of life in which they find security, are also deterrents. The study, one of the *Travaux du Centre d'Études Sociologiques*, will be of particular interest to the rural sociologist.—M. A. M.

A Survey of North West Africa (The Maghrib). Edited by NEVILL BARBOUR. Issued under the auspices of the Royal Institute of International Affairs. London, New York, Toronto: Oxford University Press, 1959. xi, 406 pp. \$5.60.

This encyclopedic volume offers the reader a comprehensive introduction to the "western island" of the Arabic world, that north rim of the African continent which includes Morocco, Algeria, Tunisia, and Libya, as well as portions of west Africa and the Sahara. The region covered reaches from the Atlantic to the western frontiers of Egypt and the Sudan and, although composed of political units that are varied in character, the region "forms a linguistic, geographical and sociological unit." Out of his long acquaintance with the Arab countries of North Africa and the Middle East, the editor, with the help of a team of experts in Arab affairs, has compiled an informative and non-controversial survey which provides not only a concise review of the history of the entire region

and an analysis of the government and political life of each unit but a careful description of social and economic conditions as well. Unique in a treatment of North Africa is the section on the French Sahara, a vast area administered since January, 1957, by the *Organisation Commune des Régions Sahariennes (OCRS)* and embracing a part of the French Sudan, Niger and Chad. The discovery of petroleum and other mineral

resources in the Sahara has given heightened significance to this region, and has injected an additional point of issue into the grave problems of Franco-Algerian relations. The status of Mauritania, Spanish-Moroccan relations, and the former Tangier International Zone receive brief but careful consideration. A useful short bibliography concludes this authoritative work.—M. A. M.

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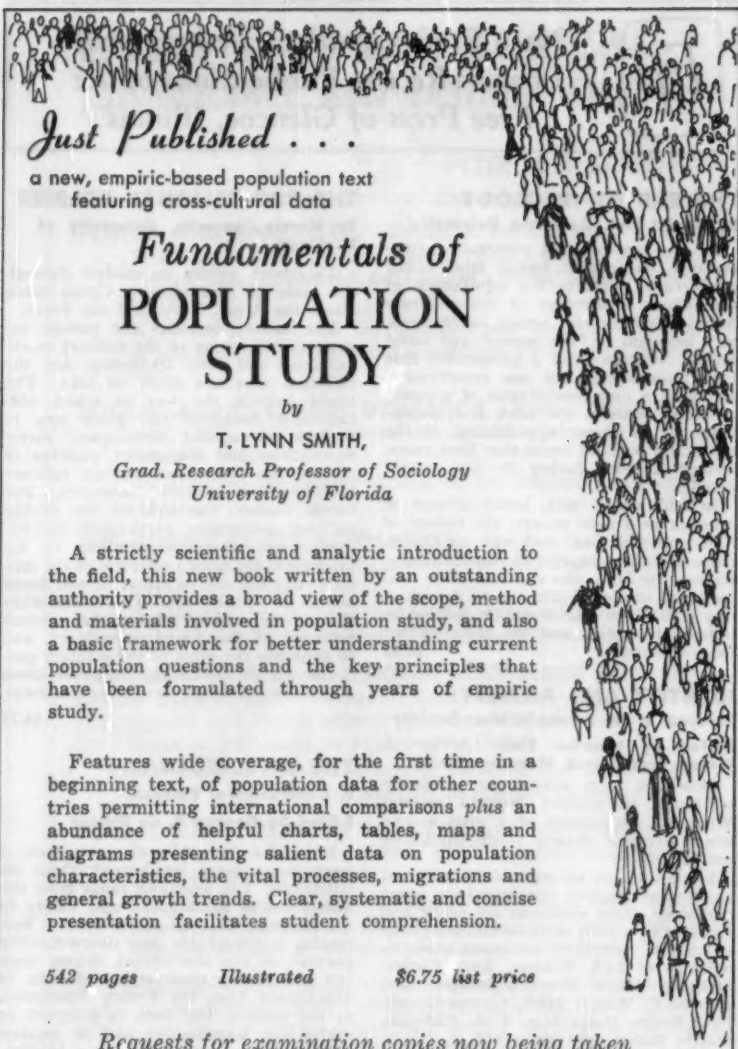
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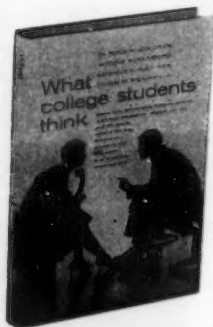
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